CUK-H06258-18-P16650

THE
BRITISH JOURNAL
OF
PSYCHOLOGY

18

CAMBRIDGE UNIVERSITY PRESS
LONDON: FETTER LANE, E.C. 4

H. K. LEWIS AND CO., LTD., 136, GOWER STREET, LONDON, W.C. 1
WHELDON AND WESLEY, LTD., 2–4, ARTHUR STREET, NEW OXFORD STREET, LONDON, W.C. 2
CHICAGO: UNIVERSITY OF CHICAGO PRESS
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THE
BRITISH JOURNAL
OF
PSYCHOLOGY

GENERAL SECTION

105
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VOLUME XVIII, 1927–28

CAMBRIDGE
AT THE UNIVERSITY PRESS
1928

P16650

PRINTED IN GREAT BRITAIN

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WANTED—A SCIENTIST

of the first order, if necessary of senior standing, but as young as possible, with a knowledge of the theory of science, to investigate and conduct the introduction of young children, 4½–10, to science and scientific method.

The problem will be at least fourfold:

THE ABILITY to absorb instruction depends on the emotional attitude of the child towards the process of being instructed as well as on the inherited quality of the brain. But the discovery of the idea of discovery and the ability to tolerate fact—which constitute the scientific attitude of mind—are the intellectual basis, on which, together with the emotional factor, subsequent intellectual progress is likely to rest.

Thus arises the need for a technique to utilise and develop the child's native curiosity in the way the wheels go round—his interest for instance in mud and water and his pleasure in messing about—in such a way as, in the long run, to obtain the maximum conversion of these drives into a controllable instrument of organised thought.

This involves the investigation by careful and delicate observation not only of what sort of activities are best introduced into the environment but what should be the order of opportunity for these activities. Much is done by leaving the child who prefers modelling with clay to heating mercury, or working a lathe to watching caterpillars or painting a table, to do so. But there is no such thing as absolute freedom and the very nature of the opportunities to a large extent limits and dictates his activities. And it is always possible—and this cannot be decided by *a priori* argument but only by observation—that to sip hastily at every flower may spoil the appetite.

¶2

IT will now be plain that this type of environment-arranging needs also the provision of specially designed apparatus. Apparatus for adolescents is too arbitrary and traditional often in the very irrelevance of its forms, is insufficiently diagrammatic, and being designed for illustration and the support of text book and teacher rather than for discovery requires—as experiments on intelligent but innocent adults will show—a pre-knowledge of its purpose. The apparatus needs to be specially adapted to the child's capacity for inference, patience and manipulation, and to be designed to meet the lack of assumptions which are implicit in our adult thinking but in haphazardly collecting which a lifetime may be consumed. There is needed a continually accumulating fluid collection of apparatus suitable for each stage of the child's mental growth, devised clearly enough to enable him to discover in response to effort the answers to his own questions. Further there is needed the verbal apparatus of explanations of the history of men's thoughts and instruments concerning the same problems with which the child is occupying himself; accounts receding further and further back into the past as the child's sense of a past matures, instead of an isolated 'subject' being worked uneasily forward to an ill-patched join with the present.

¶3

IT is as yet uncertain whether there exist any special factors limiting or making undesirable the introduction of children of 4–10 to scientific knowledge and scientific thought. That is to say whether the apprehension of multiple and permissive causality which is painful to the human mind with its innate tendency to accept and manufacture explanations in terms of unitary and magical causality, is in early life so much more painful that the forces—equally innate—of curiosity and intellectual aggression towards the external world would be stunted instead of stimulated. Or whether, on the other hand, it is not rather a quantitative question, as at present seems indicated—one of developing methods compatible with the child's childishness, with his need of phantasy, and of grading the demands of reality to his capacity.

This is the main theoretical question.

¶4

AS it is hoped that the occupant of the post will in addition to exercising and developing an art make of the task a piece of scientific work and research leading eventually to the publication of his results—negative as well as positive—he will need to make ample records. For this purpose the services of a shorthand-typist will be placed at his disposal.

Certain preliminary work with children of 4–7 has already been done at Cambridge at the Malting House School successfully enough to encourage the directors of the school to make a full-time long period appointment specially for its development.

Continued on next page

Continued from preceding page

They hope to make of the appointment the beginnings of a research institute into problems connected with education. Hence they are all the more anxious to obtain the services of someone of outstanding suitability for the work.

He would need not merely to be a specialist in his own branch but to have some little acquaintance with other sciences, the history of science and the history of religious beliefs.

It will be apparent that this type of research, more than any other, would depend for its success not only on intellectual qualifications but also on a favouring psychological background. Ideally desirable—if the view here taken of the possibilities of such research is justified—would be an immense ability to wait and see, such as would make a good field anthropologist or naturalist—freedom from irritation at the childishness of children, power to see them make false inferences and misuse apparatus, noting the facts but not being annoyed by them.

There must be an innate willingness to try to grasp the child's assumptions and to abstain from using the facile escape of verbal explanations in favour of the method of staging and re-staging occurrences till the child derives from his experience, not merely a particular atom of knowledge which will the more intimately become part of his mental structure, but the knowledge—one of the roots of understanding—of the manner in which first-hand knowledge is obtained.

Particularly in the child's early years must be eschewed the attitude of the pedagogue—the dealer in predigested reality and second-hand knowledge—in favour of that of the co-investigator, not least in order that when later in life the child needs second-hand knowledge more and more, he shall the better be able to accept and handle it.

The directors of the Malting House School are aware of the formidable nature of these desiderata. They do not however intend them as an absolute condition of the appointment but rather as an all-round view of the problem, stated in the hope that others may sufficiently share in the attitude outlined to be attracted to the work notwithstanding its difficulties. While they are willing to consider applications for the post from the teaching profession, they look rather to those who have already engaged in some sort of scientific work and whose reactions to children have not been influenced by the necessity of teaching them.

In order to be able to obtain the services of the man most suited to the work they are advertising widely and they are prepared to pay such salary as will enable him to leave his present occupation, whatever that may be. Communications should be addressed to the Directors, the Malting House, Cambridge.

Professor Sir Ernest Rutherford, P.R.S., Professor Percy Nunn, D.Sc., and Mr J. B. S. Haldane, have kindly consented to assist the directors in the final selection of candidates.

In the belief that there may be a substantial number of parents who are dissatisfied with the overworked ignorance of the majority of our schools and are at the same time anxious to avoid the emotionally-determined efforts of cranks, the directors of the Malting House School, Cambridge, have decided to convert it into a partly residential school, at present for children from 4 to 9. The Malting House School, founded as a day school in October, 1924, by Cambridge parents, is a non-profit making institution and in addition it has been fortunate enough to secure from friends of education financial support, so that it shall be run with the sole consideration of educational efficiency and progress.

The directors would be glad to hear from parents who have children of the age whom they might care to send to the School. The fees will be £115 per annum inclusive.

The following is a statement of the assumptions on which the existence of the school is based and of the methods used there:

The body of knowledge which the changing economic and social order of the twentieth century demands, particularly from the middle-class individual, is likely to increase yet further and requires for its greatest use and greatest enjoyment the backing of an organised collection of emotional and intellectual drives. It is highly probable that these drives, usually grouped together in the term curiosity, exist in the early lives of most people, and that their striking loss in later years, rendering many born with good brains intellectually ineffective and tired of life, is due to some large extent to laming by early influences.

The most consciously held aim of the educator should therefore be to avoid damaging these drives, and, lest his life should pass in loading ships with ballast, to rank that aim *before* that of the instillation of knowledge—particularly in the early years when relatively little knowledge can be instilled and great damage can be done.

At present there is no recognised, infallible or easily-applied technique for the preservation of curiosity during education. Nevertheless the directors believe that the learning of how to learn and a scientific scrutiny of familiar things, an attitude of critical curiosity and intellectual aggression to the unknown, require to be *preceded* by the discovery of the idea of discovery.

The method employed at Cambridge with children ranging from 3 to 7 to forward this result is on the one hand to eliminate the arbitrary authority of the pedagogue and to substitute for it the attitude of the co-investigator ("Let's find out" and not on any verbal information is the answer given to most questions), and on the other hand to provide an environment with more than usual scope for activity, intellectual and social, including apparatus which shall both set problems and provide their solutions. For instance: a lathe, stimulative poser of many arithmetical and geometrical questions—apparatus showing the expansion of materials under heat where nothing visible may happen except with patience—a garden with plants (which may without taboo be dug up every day to see how they are getting on, leading mainly to the discovery that that is a temptation best resisted if growth is desired)—animals which breed—weighing machines graded from a see-saw with weights, through kitchen scales, to a laboratory balance—typewriters to bridge the gap between writing and reading—double-handled saws which compel co-operation—and clay for modelling, where phantasy pays toll to skill and effort.

Complementary to, but always lagging behind this, is the attitude more and more implied for the child that "Life is too short for you to discover all that the human race has discovered before you" (this is a fact that we cannot wait for a child to discover) "and now I am going to teach you. And the things I am going to teach you were discovered in the same *kind* of way as you yesterday discovered that sand would not burn on a bonfire, that water is

Continued from preceding page

composed of those two gases you combined, and that some people, like Miss So-and-So, believe in fairies and that some do not."

The Cambridge work—admittedly limited and in a school consciously designed for children who are above the normal—goes to show that the demand for information and the receptivity to ordinary instruction is by these methods at least not lessened.

The environment may need altering. The directors claim no fixity for it. As the children grow to University age (to which it is hoped to carry them) it will constantly need adding to.

But it is to make these alterations with security that they—and other schools—need the help of central research institutes drawing in the work of investigators in correlated subjects and attached to the schools, local departments, continuously recording, testing and, it is to be hoped influencing the work done with each child.

For we need to discover no less how to reduce the psychological cost of attaining to our present standards of education, than how to produce more highly educated people.

PRIVATE enquiry having failed to produce the candidate required, the directors have recently issued on a widespread scale the above advertisement of their need for an all-round scientist—if necessary of senior standing—to investigate and conduct the introduction of children to scientific thought and method.

The object of the directors in advertising so widely has been to obtain the attention of a large number of scientists—not necessarily pedagogues—and therefore as large a range of applications as possible from which to make a selection. They hope also by stating publicly their view of a problem, which has not yet received sufficient attention, to precipitate the attention and agreement, disagreement or co-operation of all those—scientists, parents, educationists or members of the general public—who are interested in its solution.

☆ ☆ ☆

WHILE visitors to the school are welcomed, it may be mentioned that a week's activities of the children have been filmed (the children being "stalked" whilst engaged in their everyday occupations). A private view of the film is shortly to be given in London, probably on a Sunday afternoon. Those who would like to see it are asked to send their names and addresses to the Directors, the Malting House, Cambridge, and a formal invitation will in due course be sent to them.

Publishers: *Williams & Norgate*, London *G. E. Stechert & Co.*, New York *Felix Alcan*, Paris *Akad. Verlagsgesellschaft*, Leipzig
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VOLUME XVIII JULY, 1927 PART 1

THE RELEVANCE OF VISUAL IMAGERY TO THE PROCESS OF THINKING. I

BY T. H. PEAR.

(*From the Psychological Department, University of Manchester.*)

1. *Introduction* (pp. 1, 2).
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3. *The development of visual images as a vehicle for thinking* (pp. 5, 6).
4. *The visual image's 'irrelevance' and 'discrepancy'* (pp. 6, 7).
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6. *The use of words in thinking by the visualizer* (p. 9).
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10. *Evidence of deprivation* (p. 14).

1. *Introduction.*

THE difficulty of bringing an indictment against a nation is notorious, yet it does not prevent one from believing that the representatives of certain countries have tastes which differ from one's own. And so, though I know many philosophers and psychologists do not hold the views discussed in this article, those who do seem numerous enough to justify this attempted statement of opinion.

In this paper will be expressed the difficulties which prevent me from believing that visual images are necessarily lowly vehicles of thought and that they are often irrelevant to or discrepant from the thought which proceeds independently of or even hampered by them.

Now, though I believe that the visual image has been scurvily treated of late, I do not regard it as a Dulcinea. And this attitude is strengthened by a distaste for charging philosophical windmills. I will therefore try to describe what I believe to be misunderstandings implicit in the modern treatment of visual imagery. Explicit misunderstandings will not be discussed here.

The symposium is definitely restricted to the consideration of *visual* imagery. With visual imagery I have constant daily and frequent nightly acquaintance. Of imagery other than visual I shall not write here, not knowing if a visual image's production is governed by the same laws as those which underlie the coming-about of the auditory image. I see few reasons why they should, and several why they should not be identical.

The following considerations will be suggested:

(1) That the characteristics of visual imagery are extremely numerous and varied.

(2) That its development as a vehicle for thinking passes through different stages, that these stages ought to receive different names and ought not to be confused.

(3) That, while at some of these stages the visual image is disadvantageous for efficient abstract thinking,

(4) other ways of arriving at truth along visual routes may be uniquely efficient, and that this fact is more familiar to artists than to psychologists.

(5) That considering the patchy growth of recent psychology, it is quite credible if less creditable that workers should differ about the 'irrelevance' or 'discrepancy' of visual imagery.

(6) That to the results of some experiments investigating the usefulness of imagery in thought an unjustifiably wide significance has been attributed.

(7) That in considering the use of visual imagery the 'biological importance' of the situation in which it functions should not be neglected.

(8) That in many experiments, the situations were already 'half-abstracted,' and so might discourage visual imagery even in a visualizer.

(9) That the apparent irrelevance of their imagery is sometimes experienced by visualizers themselves. Yet this irrelevance raises problems which require for their solution the concept of the unconscious.

(10) That the distinction between 'concrete' and 'abstract' imagery is not helpful.

(11) That the nature of visual imagery makes it difficult to record, that this hindrance may be minimized, with results which will be shown.

Let us now consider these subjects separately.

2. *The numerous and varied characteristics of visual imagery.*

With some exceptions, writers on the functions, as distinct from the structural details of imagery, do not treat fairly the several attributes of visual imagery, which differ greatly in individuals. Visual images may vary in frequency, clearness, quickness of recall, and (not so easy a matter as may appear) relevance or usefulness. In the Galtonian days, clearness was the criterion most obvious at first sight. And many statements about imagery in general relate implicitly to clearness.

Now the correlations between these several functions may vary infinitely. For example, a clear image may be useful because of its clearness. Sometimes this property may stultify thought. And occasionally the unclearness of a visual image cruelly depicts this attribute of the thought which it symbolizes.

Other attributes of visual imagery are important variables. Such is the interest or emotional colouring (possibly attributable to bodily factors at different levels of complexity) which seems to determine whether an image shall stay in consciousness, and if so, in what direction it shall develop. Such interest seems to be important in encouraging or discouraging the appearance of colour, form and movement in the visual image. Whether such interest is less or more potent than 'natural capacity' is difficult to say. Possibly this is the problem of the hen and the egg in a new form.

A personal example may illustrate this. Colour in my images is usually very fleeting and faint, as if the sun had momentarily lighted them. My first winter sports holiday left behind it an amazing crop of imaged colours, vivid, lasting, interesting, and relevant to a degree greater than I ever remember before. Sometimes, too, the images were (as they are while I am writing, fourteen months later) semi-abstractions of colour from form; vivid patches of colour with no well-defined shape; serving merely to identify the various pictures of my chromatic acquaintances. And this should be an important point for those who write about 'concrete' imagery. Apparently my casual and fleeting colour images were unworthy to represent this dazzling Alpine and aniline triumph. Perhaps enhanced images emerged from the fusion of these two mighty forces.

Personal interest, then, seems to be very important in this question of imagery, a matter perhaps easier to keep in mind in the Alps than in the laboratory.

Looking at this matter from the opposite end, laziness (and this verbal hold-all hides a heap of psychological odds and ends) seems to discourage a visual image from staying or from developing. There is a popular belief that a visualizer should be good at card games, "because it is so easy for him to see in his mind which card is out." I should not be surprised to find that the correlation between the ability to visualize concrete scenes or faces, and that to remember which playing-card is out, is low. For this is just one of those jobs which can be done well, or better, by words, or even by imageless thought if such a lofty mechanism be mobilized for such a simple matter. The visualizer may prefer to use verbal recall in such a performance. For continuous visualizing is a strain, while repeating verbal formulae is not; a truism sometimes forgotten in politics and psychology.

This encouragement of images by interest is also true of visual images of movement, though such a phrase is ambiguous. It may mean (*a*) visual images in which the movement is actually seen taking place, (*b*) a static depiction of some phase of the movement, as if the picture were cut from a cinematograph film, or (*c*) a felt movement of the eyes over the image. (*c*) may occur with either (*a*) or (*b*).

Now since the image may show differences in all these attributes we cannot be too sure about its limitations. That such imagery can be cultivated seems probable. Since my interest in human movement has increased I enjoy visualizing a human being in movement, instead of getting a slow succession of static images of the performance. Yet often this pleasant aesthetic appreciation of a very concrete visual image is based upon knowledge gained through more abstract images or words, *e.g.* through notations or diagrams. These are the indispensable raw material out of which the new image grew.

Now this offers an interesting problem. The image has surmounted the words, or even the imageless thought. For aesthetic pleasure from imaging the apparent simplicity of certain complicated movements implies a previous analytical knowledge. This knowledge involves references to gravity, to friction, to the parallelogram of forces, to the art which conceals art. So perhaps this naïvely concrete image has had a philosophically respectable ancestry, even including an occasional strain of pure thought. Far from being a lowly hanger-on, it may even, like a commanding officer on parade, 'take over' when words (the N.C.O.'s) and thoughts (the captains) have completed their subordinate tasks of collection and formation.

These examples of colour and movement in the image illustrate how its contents may become specialized, abstracted, emphasized, skewed, thrown into a new focus or put into a new perspective. As a result of any or all of these processes its meaning may be refined, deepened, enriched, accented. Visual imagery has its own technique of elaboration and abstraction.

3. *The development of visual images as a vehicle for thinking.*

There are probably many ways in which the contents of a visual image change so that it may function more effectively as a vehicle for thinking. The following are only approximate descriptions of them. Visual images may become (*a*) attenuated, (*b*) enriched, (*c*) the vehicles of another meaning, (*d*) structurated, (*e*) associated with dispositions underlying emotions, sentiments, complexes, or constellations other than those with which they were originally connected.

The attenuation of visual imagery is shown in the tendency of some minds to visualize diagrams for the appreciation of complex matters, these diagrams appearing non-voluntarily. When I think of a writer ignoring one half of a subject my image is of a circle, half-black and half-white. But to me, since the image means hemianopia, it needs no words.

Enrichment is one result of Freud's 'condensation.' Two images may fuse, but, unlike their perceptual prototypes, sometimes they can be kept apart and at other times they cannot. As I write, I have a very clear visual image of a scene, but I know it to be partly condensed with a striking photograph, which I often see. At present it is difficult to dissociate them, but at other times I find this possible. I have seen scores of different photographs of a famous actress. But they do not interfere with a very clear visual image of her as she was when I saw her act. (They did not do so while I was writing, but on revising my manuscript the confusion between the images had increased.)

To a visualizer the fact is familiar that a visual image may become the vehicle of a subsequent different meaning. I have before me two photographs of scenery, which I can image vividly. The first, taken in brilliant sunshine, shows all the shadows as sharp black patches. The other is a symphony in grey. I have noticed that these two pictures, originally bought to remind me of favourite scenes, appear, together or apart, as images when I am thinking about delicate beauty or photographic technique. This was made possible when these and other relationships between the two pictures were seen.

Sometimes this shift of meaning seems to be symbolized by a shift of the spot-light, so to speak, from one part of the image to another. And this process underlies a certain type of wit and of invention. But when this shift is directed successively upon different *related* features of the image, its meaning thereby becomes more general. Probably some images are especially suitable material for this process.

After many such changes an image may represent a vast structure of meanings, and so 'structuration' may be attributed to it. Moreover, at times when the emotional significance of an image changes it may become the vehicle of an entirely different kind of thought.

4. *The visual image's 'irrelevance' and 'discrepancy.'*

Writers about the human mind take very different views of psychological relevance. Irrelevance seems scarcely to exist for Wordsworth and for Freud. To the behaviourist, most things are irrelevant. Yet we should not be encouraged by this difference of opinion to ignore the problem, as it arises in considering images. Visualizers occasionally experience images which they would call irrelevant. In myself, some hypnagogic images appear to be quite disconnected with my everyday life. During a period of insomnia I used to know when I was falling asleep because then my images changed in character, seeming to have nothing to do with everyday worries and desires, while the ordinary images, which seemed to keep me awake, were pictures of them. The 'foreign' images were usually of demons or fairies, in miniature, often dancing as on a stage. Though clearer than my ordinary images they were never taken to be percepts.

It is possible that analysis, connecting these demons and fairies with deep-lying complexes, would show their relevance. Yet I would urge that the apparent irrelevance of visual images is admitted by the visualizer. To consciousness the images are irrelevant while they may be unconsciously relevant.

Ultimate causes form no part of the material for this discussion. But certainly one root of the idea of discrepancy or obsoleteness is to be found in Galton's *Inquiries*. He records his surprise that geometricians who, one might have thought, would naturally have employed visual images, did not do so, or had outgrown them. From this one might surmise that the use of geometrical figures in experiments upon visual imagery may encourage the *non*-visualizer.

And Napoleon's famous dictum[1] has impressed many, that "those who...form a picture (*tableau*) of everything...are unfit to command." But if Napoleon said exactly that, did he mean more than that he had no use for introverts? Dr Henry Head records that some of his intelligent air-force officers were visualizers. Napoleon might have found visual imagery more helpful in an aeroplane than on a horse. And undoubtedly the remark contains the vicious 'faculty' assumption that any visualizer is equal to any other visualizer.

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[1] Quoted in *Inquiries into Human Faculty*, London, p. 78.

Binet's picturesque assertion "With a thought of a hundred thousand francs one has images of four sous"[1] is also famous. But to use images in a war on images is to invite reprisals.

The famous experiments on thinking, by the Würzburg school and others, have shown conclusively that under certain conditions, subjective and environmental, certain types of mind can solve certain types of problem without the aid of images or even, sometimes, of words. But it is fair to remark that the problems were abstract, so that in them much of the important working-up of the raw material had already been done. Some of the observers were famous for such thinking performances and even the student observers had been already skewed towards philosophy. Often the solutions of the problems set were not biologically urgent. Furthermore, Miss Claire Comstock's experiments[2] demonstrate the impossibility of assuming that a person who habitually uses visual imagery in abstract thinking would have solved these problems without such aids, or that such a person could have solved them at all.

5. *Concrete and abstract imagery.*

A distinction which may be attractive before it is applied to actual examples is that between 'concrete' and 'abstract' visual imagery. But it confuses the content or structure of the image with its function. Even the most definite or concrete perceived object may carry either a concrete or an abstract meaning. A masonic symbol, for instance, is as concrete an object as can be. But to many uninstructed persons it represents only the general meaning of brotherly goodwill. To a widow who has been helped by the organization, the symbol may carry the most concrete, poignant personal meaning.

This dual function of the physical object is matched in the image. 'Abstract' images may carry concrete meanings, and *vice versa*. Many visual images, even if 'photographic' (whatever that may mean, for even the camera abstracts) or 'mimetic' (and so notoriously, does the mime), may carry the most abstract or the least abstract of meanings.

This seems to be the place to record my difficulty in understanding what is meant in the common belief that an image *copies* an object. My visual images are all abstractions, in the sense that they do not revive all the visual aspects of the original perception. In those which are biologically significant, this abstraction has usually gone far. Moreover, often this abstract image, when it appears, has fused with others whose meaning has also been fractionally distilled. Yet the *meaning* of such a resultant image (twice or thrice or more times removed from reality) may be either concrete or abstract.

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[1] *Année Psychologique*, 1911, p 10.

[2] "On the Relevancy of Imagery to the Processes of Thought," *American Journ. of Psych.* 1921, xxxii, 196 f.

As I now write, an image of the Jungfrau appears with silver gleams on its graceful shoulders, and an evening star swung high above a crag like a castle in the *Ring*. In one way this image outstrips the original perception, for it defies the cramping laws of binocular vision. From that spot an eagle might see the Jungfrau's front and this crag together; no man could.

The details of this image—a memory of leaving Wengen at sunset—are thrillingly concrete. But its meaning that evening was Wagnerian; one was leaving gods and gnomes. To-morrow its meaning may taper to a point—if I go south, shall I change at Spiez?

As I made rough notes for this paper, an image appeared of the face of one of the most beautiful persons I have ever seen; unusually beautiful too, because it was an idealization of many other images. At that time it appeared with a mysterious imaged background of a dark cave; my train was passing through the rock-cuttings in Derbyshire.

Now both these images carry complex meanings. The first meaning admittedly has reached a high level of generality. The images represent in different ways the mystery of superlative beauty. The first meaning is obviously due to the mighty abstracting powers of Wagner; the second to painters and poets innumerable.

The verbal thinker might express these experiences in a few phrases. Maybe (I cannot know) his phrases will bring as much pleasure as my images. And he can share the sound or sight of them with others; if I could share my images I should not be writing about them. But if our problems in thinking-experiments are set in words, are we not investigating special, though lofty, types of abstraction, generalization and thinking? I am not unmindful of the careful investigations of the process of abstraction in which words were not the only material used. But (*a*) the problems were usually chosen for the subject, and did not arise out of his personal private needs, (*b*) some of the material used, such as diagrams, would not appeal to some visualizers, being more attractive perhaps to non-visualizing geometers. For some visualizers are bored by diagrams which they have not invented themselves, just as they resent illustrations in a novel. They are perhaps more interested in their own visual abstractions than in other people's.

Now the importance of abstracting a general meaning from specific instances is undoubted. Yet one way of amplifying this is to say that the determining tendencies underlying the abstracting process are all-important, and that whether they eventually issue in a word or a visual image is comparatively unimportant. I cannot see how this opinion is reached, for the differences between the functions of picture-vehicles and word-vehicles seem to be very significant.

6. *The use of words in thinking by the visualizer.*

Even the extreme visualizer occasionally performs what has been impolitely termed 'sub-vocal muttering.' But it feels quite different from his usual thinking.

It is naturally not easy to observe oneself while thinking. But I notice that sometimes I say words with vocal or sub-vocal muttering, when attempting to strengthen an intention. Recently I found myself formulating, 'Take a line and stick to it'; occasionally I seem to hear, sub-vocally, 'Suaviter in modo....' But both these are examples of situations in which a previously-made determination needs stabilizing. And at such times M. Coué's beliefs are easier to grasp.

7. *Evidence of the value of visual imagery in carrying meanings, and of its effective attenuation with the passage of time.*

It has now been admitted that the visual image is of inferior value for some problems which some philosophers, on their lawful but not always urgent occasions, have set themselves. For other kinds of abstraction, however, they may perhaps be uniquely useful. Instances will now be offered in which they seem to function quite effectively.

In the following records there are perhaps some relatively novel features. The experiences were observed under very favourable conditions. They were of the greatest interest, both personal and scientific, to the recorder. The motives for wanting to utilize the knowledge carried by their representative images are very cogent. By the aid of a dictaphone, the use of which has become habitual, the details concerning the images could be dictated with unusual rapidity, and without the disturbing presence of a second person.

(July 8th, 1926.)

On July 1st, 1926, from 2 to 4 p.m. I visited the Wimbledon Lawn Tennis Championship contests for the first time. I could stay only two hours. My interest was chiefly psychological, and I was less attentive to the outcome of the games or the individual scores. I wished to appreciate the play and, so far as possible, to see the bad players' faults focused against the relative perfection of the Wimbledon performances.

Three points emerged very clearly in memory. One was the fault of standing still after the service, so that one has less time to 'get a move on.' Possibly some good players succeed if they stand still: those whom I saw did not. They moved often towards the net; sometimes in another direction. This was illustrated by the fact that if they served a 'fault' they always had to return to the base-line, sometimes as far as ten feet.

The second point (an obvious one, but very strikingly so at Wimbledon) was the suicidal effect in a 'doubles' of leaving any part of the court open, especially against the Americans.

The other useful memory was of the very extensive preliminary swing-back of the racket before making a back-hand drive, even when to a novice it seemed excessive.

Now these are all commonplace facts to good players, probably even to readers of books about the game. Why are they brought forward here? Because these three object lessons were carried, hours after I had left Wimbledon, in vivid visual imagery of the players actually making these strokes. It might be objected that since I have the knowledge, I could carry it in words, as indeed writing it down must necessitate. This is true, but in actual fact it was carried by visual images which, seven days afterwards, were still very clear and insistent. It was then that with the dictaphone I made a record from which the following passages are literally transcribed (with no alterations of grammar or style).

I will now describe, as photographically as possible, without any reference to the function of these visual images, those which arise when I shut my eyes and think about the play a week ago at Wimbledon. It is obviously necessary to hold one image to the exclusion of others as they tend to press in.

(1) I am standing at or near one corner of the Centre Court. I *know* that the grand-stand opposite is packed with people. Nevertheless it is an entire blank of black, as if it was night, while only here and there, there are whitish blotches suggesting the presence of people....

Next comes the net, very vivid and tightly stretched, and the groundsmen slowly retiring after having tested its height. Mademoiselle Vlasto is at my end, Mrs Godfree at the other. Immediately they change places, Mrs Godfree is at my end and I cannot see her opponent. A very white ball flashes backwards and forwards in the middle of a visual image which consists now of nothing but a (word lost, probably 'schematic') net and a grey darkness. The pace of the ball and its angle are very striking. I believe the pace of the ball is not exaggerated but is exactly the same as at Wimbledon. Passes between two very vague rackets with no suggestion whatever of any players at all. Now I can see Mademoiselle Vlasto focused very clearly against the blackness of the grand-stand. Her brown arms show out, in very clear detail. Now Mrs Godfree replaces her: a flashing image of the upper part of her body shows the tremendous swing back of her right arm in the forehand drive.

The next picture is one of Lycett and Austin on my side of the net, Austin caught out of position while one of the Americans makes a perfect 'railroad' drive, just skimming the net with the greatest ease. The same ( ) is carried by a picture of Lycett looking physically distressed, and racing back to back up Austin and nearly colliding with him. In this picture the sun is blazing vividly on the players, and though they seem to be very near to me, at the present moment, neither of their faces really corresponds with the actual players. They seem to be mere (? schematic) figures to point a moral....

Kinsey's superbly cool method of walking across the court is now shown by a vivid and accurate picture of him (eyeshade complete) walking with the slightest suspicion of a swagger. Then suddenly he makes a perfect back-hand drive almost along (?) the next court from left to right. The sun blazes out and the next picture is of a lob twinkling high above the grand-stand.

Service is very clearly illustrated by Miss Ryan's characteristic movements. The image now consists chiefly of Miss Ryan's arms up in the air, then they vanish and a racket swings across from left to right. Then a schematic image of Miss Ryan's quick dash to the net and rapid alternation of balance from one foot to the other. Then a vivid image of the court ( ) and (this apparently the first time since I began to describe) of the stand, with as many people as there were at the time. Then away on the right in Court No. 1, the score-board, the names J. Brugnon, Cochet, J. Borotra and a long French word which seemed to contain about nine letters and the syllable -ing.

At no time can I visualize the four Frenchmen anywhere near the base line. Probably for the sufficient reason that they were never there as far as I remember. But a very clear image of Borotra waiting for a chance, which he eventually gets, to drive hard between his two opponents exactly across the centre of the net. This image occurs several times, from which I assume that it may have happened several times in the game.

(2) My imagery to-day differs from that at midnight, or thereabouts, on July 1st in that it is to-day far more under control[1]. I can select from a number of images, all searching for admission to consciousness, with greater ease than a week ago. Then they were insistent, vivid, exciting, and much fuller of actual movement. If I had been in the dark while having them, I think I might easily have been troubled by them before going to sleep. The bedroom, however, was not dark and the conditions for a hypnagogic image were not favourable. In any case, as far as I know, this imagery did not prevent me going to sleep. But I remember that it was more vivid and less schematic. For instance, I remember that I felt I could have painted or modelled Mademoiselle Vlasto or made a carving in ivory. To-day any image of her is partial and more like a diagram. Last week the people on the grand-stand and their vivid dresses in the sunshine (I was in the shade looking over towards the sunny side) were very much as in reality. The sparkle of the new balls, something which I have never seen in tennis before, which was also noticed that day by a newspaper critic) played a great part in the images a week ago. Unless I wish to bring it up, it plays no part now. The facial details, as for example of Lycett, whose face I can visualize very easily because he is like a friend of mine who is about his own age, were then very clear. Also the faces of Kinsey and Richards. *Now* I can say that both these Americans are caricatured by my image. Kinsey is very little more than a lean face

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[1] *Comment on this description:* It is obviously open to criticism to offer as evidence my memory on July 8th of the images as they presented themselves to me on the evening of July 1st. My images on July 1st were so vivid, impressive, and in certain respects so unlike those on July 8th that on the latter date a comparison forced itself on me which I also recorded by dictaphone.

with spectacles under an eyeshade, a tightly belted waist and a way of walking which is American rather than English. Richards's boyish face is now puffed up. Mrs Godfree is chiefly a large white jockey-cap and some extraordinarily active legs and arms. In almost every case, however, what remains is the distinctive feature of the player, what has gone is what they share with others. For instance, I doubt if I could visualize Borotra without his round cap, or Miss Ryan without her muscular arms and her nervous way of dancing about the court.

I should say then from this and other experiences, that my visual images a week later have lost in detail, but the detail which has been lost is very frequently irrelevant to the purpose. They have become more diagrammatic. It is quite possible that this selection has been caused by my putting my knowledge into words. But this seems to me only to fix the knowledge in a way in which it can be more readily reproduced, by myself or others, and criticized.

What seems to me to be important in the present case is this. These visual images give a very intense real colouring to the past experience. They are the way in which such lessons appeal to my particular type of mind. It is quite possible (though I think far from certain) that this is an inferior way. Even I, when I can, put such knowledge into diagrams, notations or words. But I do not believe that criticism of the use of visual images by persons who either do not possess them or do not make use of them, is very helpful in the present case.

X has often urged that I should give an account of the memory of something which really matters, and really has practical results. This seems to me to be one.

8. *The image as irrelevant to or discrepant from the process of thinking.*

Some psychologists, who believe the value of the image in thinking to be small, write of it as irrelevant, others as discrepant. Webster gives the meanings of discrepant as "discordant, at variance, disagreeing, contrary, different." Speaking only of my own visual images, I doubt if any (excepting possibly hypnagogic images, which seem to be of another type) appearing at first glance to be discrepant or irrelevant are really so. When examined, they prove to be closely associated with the subject of thought. But this matter has been exhaustively treated, both theoretically and practically, by Comstock[1]. Her conclusion was that there is no irrelevant imagery, and the reader is referred to her paper for answers to many questions which the present article may seem to raise.

Moreover, psycho-analysis has made it impossible for us to be satisfied with the fact that images at first glance appear discrepant. Everyone who relates a dream notes images that appear to be discrepant. But it was with some such fact in mind that Freud wrote his *Traumdeutung*. And until the writers on thinking and the writers on dreams acknowledge each others' existence, this part of psychology will continue to progress with an ataxic gait.

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[1] *Op. cit.*

9. *The usefulness of visual imagery.*

Evidence is therefore available from experiments like those of Comstock, from the findings of psycho-analysis and from records such as those on pp. 10–12, that visual images are generally relevant and useful, and that in some types of abstraction they may be uniquely useful. Yet the experiments of Dr Betts[1] and of Dr Nellie Carey[2] are sometimes cited as showing the uselessness of visual images in problems where their utility had formerly been presumed. But concerning these experiments I would urge certain considerations.

Betts' subjects were, for example, asked to answer questions about a cube painted red, sawn into inch cubes. Carey used drawings, consisting of five or six straight lines, and cut holes in folded paper. And both these classes of material are more than half-way to complete abstraction. Even the visualizer would use words about them.

Dr Carey's experiments also seem to have provoked the primary memory image. The results are significant, yet the 'primary memory image' is not a *memory* image in the ordinary sense. It is a transition between perception and ordinary mental imagery, a distinction drawn by Jaensch, Allport[3] and others. Dr Carey cannot be reproached for not knowing about the eidetic image, which was not reported until fairly recent times. But the relationships between any person's eidetic or primary memory images and his highly structurated memory images, occurring at a much later time after the perception, cannot be assumed.

The results of both Betts and Carey are not surprising. I have often noticed that problems which could be solved either visually or verbally were solved by using words, and believe they were often done more easily through words. But these problems were abstract. A visualizer may be bored as stiff as anyone else by five or six straight lines joined in a variety of irregular ways; why not? And, like the verbalizer, he may not visualize the shape of a hole cut in folded paper, not because he cannot, but because there are less strenuous methods of attaining his aim.

I would suggest that in devising material for experiments upon imagery and mental tests the bias of some investigators towards geometry has already gone too far. To assume that a person's ability to deal with abstract problems can be measured by his memory for, or intellectual manipulation of, geometrical figures invokes the doctrine of 'faculties' too light-heartedly.

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[1] *The Distribution and Functions of Mental Imagery*, 1909.

[2] "Factors in the Mental Processes of School Children." This *Journal*, 1915, VIII, pp. 70–92.

[3] This *Journal*, XVI, pp. 99–120.

10. *Evidence of deprivation.*

In Martin's experiment[1], where 150 cards were shown to five subjects, the subjects were told to suppress all images altogether. This, it is reported, they found themselves quite able to do.

It seems likely that if these problems were half-abstracted, if they did not matter much to the subjects, or if the subjects were not habitual visualizers, the images could quite easily have been suppressed. It is equally reasonable to urge, both from introspection and from acquaintance with normal and abnormal visualizers, that 'quite easy' suppression does *not* succeed with biologically urgent images.

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[1] *Zeitsch. für Psych.* LXX, 1914–15.

THE RELEVANCE OF VISUAL IMAGERY TO THE PROCESS OF THINKING. II

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So many interesting and important points are raised in the preceding contribution to this symposium that it will be quite impossible for me to deal with them *seriatim* in my own. I shall accordingly limit myself strictly to those which have a direct and immediate bearing upon the subject of our discussion as indicated in its title. If it should thus appear that I am neglecting outstanding trees which I ought to examine one by one, I shall plead that I am envisaging the wood as a whole, as I see it through the wealth of Prof. Pear's detail.

Unlike Prof. Pear, who appears from the careful introspections which he gives to be singularly privileged in this respect, I do not normally enjoy visual imagery in thinking; nor can I call it up at will. Yet I feel I have some right to take part in this symposium; for I may say that I know what it is to experience visual imagery, since the dreams I remember are often sufficiently vivid to be confused with actual visual perception; and, moreover, of imagery of other kinds I possess a fair abundance.

But it is not, I take it, merely by reference to Prof. Pear's imagery or to my own lack of it, that the whole question of its relevance to thought process can be definitely decided. For relevance is an ambiguous term; and our problem resolves itself not merely into one of the relativity, applicability, pertinence of image to thought, but also into that of assistance or helpfulness. These two meanings of the term are not infrequently confused with results disastrous to clear thinking. If, when we think an object, we have also an image of it, obviously we can say whether the thought and the image are relevant one to the other in the first sense of the word. That is, we can educe the relation of applicability or pertinence from the two fundaments, thought and image; or we fail to educe it. But it is quite another matter to say, on the strength of introspection alone, that the image helps the thought; or, for the matter of that, that the thought helps the image. This is a question of function; and I do not think that introspection throws much light here.

Indeed, both parts of our problem—the pertinence of visual imagery to thought and (as I conceive it) the far more important question of the rôle played by imagery in thinking—require more than the introspections of one, or even of several individuals for their solution. These, it seems to me, are matters for statistical investigation. The occurrence of visual imagery, with regard to which there are great individual differences, is one thing. Its relevance, in the sense of pertinence, to thought is another; and here again individuals may greatly differ. Its precise function in relation to thought is yet a third. Perhaps individuals differ also in this respect; but, in any case, this point seems to call for an objective method of investigation. Introspection is necessary, but not sufficient for its elucidation.

It may be that visual images are not only pertinent to thought processes, but even that thought cannot proceed in their absence—that they are a condition *sine qua non* of all our thinking.

Such a view, at any rate, has been held by a numerous and quite respectable body of philosophers which has shown a very remarkable continuity since the time of Aristotle—indeed, allowing for differences in theory, one might even say Democritus. If it be the case that we are incapable of thinking without images, then clearly the images are relevant to the thoughts, whether they actually *are* the thoughts themselves or only the instrumental causes by the help of which thoughts are produced. In either case, I should be obliged to explain my own personal experience of thinking by reference to imaginal processes so weak as to be unintrospectible. Indeed, many modern psychologists do explain thinking precisely in this way. Thought, for them, is the incipient awakening, or sub-excitement of images.

Both the ancient philosophical doctrine and the modern psychological explanation in question are *ex parte* hypotheses made to fit in with metaphysical views as to the nature of perceived and thought objects on the one hand, and of the conscious subject on the other. They can be properly understood only in the light of the historical ideology of which they are the offspring, and with the surmise that those who framed and defended them were what we now call 'visiles.' Were I a 'visile,' like Democritus, Aristotle, Aquinas or Prof. Pear, the view might appear to me to be plausible. But, unless in my own case I am to begin my psychology with an epistemological postulate, I must take consciousness simply as I find it. And, if I discover no visual images in the course of my normal thinking, clearly for me visual images can have no pertinence to my thoughts, nor can they be said to function in their regard. On the other hand, I know what pertinence is, for example, as between a kinaesthetic image and a thought; and I am quite ready to admit that there may be an extremely high degree of relevance of this sort between visual imagery and thoughts in a similar manner.

To show, however, that there is relevance of the other sort I believe it would be necessary to carry out investigations in which one could obtain objective records of the products of thought in those cases respectively in which visual imagery was present and absent. Moreover I should not consider such investigations to be complete until they had been carried out with regard to all the characters of visual imagery—such as frequency, intensity, determinateness, etc.—some of which Prof. Pear has distinguished. To some extent investigations of this kind have, of course, been carried out. Two outstanding ones (criticized by Professor Pear on the ground that the material used in them was already more than half-way to complete abstraction) conclude in general that, apart from inner speech, spontaneous imagery is negligible, that it arises often only after the thought, that it is frequently irrelevant (Betts); that in school studies imagery may be detrimental, and that there is a tendency to inverse correlation between some of the higher mental processes and imagery (Carey). These investigations are admittedly not exhaustive; but as far as they go they do seem to point to irrelevance in both the senses in which the term is used.

Though he offers criticism, Prof. Pear is inclined to concede many points; for he states that in some of the stages at least of its development the visual image is disadvantageous for efficient abstract thinking. And he says that it is often easier to solve problems, which could be solved either visually or verbally, by using words. Indeed, in a passage which has greatly puzzled me, he seems to go still further when he refers to an image as having 'surmounted' inner speech or even imageless thought. Far from functioning here as relevant or ancillary to thought, the image seems to be the product of the thoughts. It can be said to be relevant in the first sense, but certainly not in the second. I cannot at all agree, if I understand it aright, with the transition made by Prof. Pear from this account of the genesis of the image in question to what follows: "Far from being a lowly hanger-on, it (the image) may even, like a commanding officer on parade, 'take over' when words (the N.C.O.'s) and thoughts (the captains) have completed their subordinate tasks of collection and formation." I shall not cavil at the use of the terms 'lowly hanger-on' or 'subordinate tasks' in this connection, any more than I have cavilled at the idea of solving problems by words or images. But

I cannot see that this highly picturesque description helps the point that is being made. For I should regard the image resultant from such a process of thinking as at most merely illustrative of it.

There is another way in which I believe imagery may helpfully be investigated—the indirect way, namely, of investigating some 'higher' thought process and ascertaining the manner in which imagery enters into it. In so far as I may claim to have studied imagery experimentally, this is the method that I followed in obtaining the data for my *Consciousness of the Universal and the Individual*. The conclusions I was able to draw in that work have largely been corroborated, and in one very significant respect traversed, by the results of a similar, and quite remarkable research lately carried out by Stevanović in the laboratory of King's College. This research is now in press, and will shortly be published as a Monograph Supplement of this *Journal*.

My own work and that of Stevanović were both concerned with judgment. In both researches, after associating nonsense words with sets of pictures by learning them together, these nonsense words, now having acquired some meaning (viz. meaning the associated pictures) were used as grammatical subjects to which the observers were required to fit appropriate predicates. Apart from the differences in aim of the two researches, one of the chief differences between the two pieces of work lay in the nature of the material learned. In both cases it was visual material; but in my own research this consisted of pictures of familiar objects—small children in movement, fruits, and the like. Stevanović worked with what might almost be called nonsense material. He used 'pictures' obtained by throwing shadows of cards held at various angles to the source of light. These shadows he outlined and washed in with colours to most of which names could not readily be given. The resultant 'pictures' thus consisted of coloured shapes sufficiently dissimilar to cause a real difficulty when one attempted to derive a common concept from them, yet sufficiently alike for this derivation to be possible.

Now, before citing any conclusions from these two researches with regard to imagery, I wish to say that I consider the percept itself as the most perfect kind of visual image. This involves a view which I need not develop here. Further, if I am thinking about the percept, this image seems to be entirely relevant, both as pertinent to the thought and as helping it. It seems to be a πού στῶ for the thought, to arrest or hold it in its process of perpetual becoming. It is, then, in reference to the percept or direct image, that images and their relevance must be judged. When an object seen is recalled in a sensorial manner, the image is known to be complete or fragmentary, vivid or obscure, and the like, by comparison with the original experience, which is now a thought and not an image. Its relevance (in the first sense) is judged accordingly. It is a copy or picture, a schematic representation, a symbol, or the like. Belief in relevance in the second sense, we may surmise, is a consequence of knowledge of relevance in the first sense. We are thinking about an object; at the same time we are experiencing an image which we know to be a copy, representation or symbol of it. But we do not, when thinking an object or about one, always experience such an image. Or, we may think an object or about one, and experience the image of another. It seems to me clear that, just as one may have an auditory image (say, of a tune) while thinking of a place, so one might have a visual image of one thing while thinking of another. Indeed, we can actually perceive objects (experience direct images) while thinking of others. Such images and percepts could only be held to be relevant (in either sense) by way of some association, or in some such 'psycho-analytic' manner as that to which Prof. Pear makes appeal. But, in that case, everything in the way of imagery could be held to be relevant to anything in the way of thought; and there would seem to be very little chance of a scientific determination of the matter. For it might be said—as, indeed, it has been said—that any content of consciousness can mean anything whatever. I do not wish to minimize the importance of association, or to speak with the slightest disparagement of the excellent work of the psycho-analysts; but I do believe that we must find somewhere within consciousness the complete grounds for all the concepts which we frame to explain its processes. And I believe further that, from the standpoint of consciousness, it is easier to accept the frequent apparent irrelevance (both as pertinence and helpfulness) of thought and image than to call upon association or the unconscious to establish a relevance where there seems to be none.

To return from this digression and mild polemic to our researches, I may say that my own conclusions as to the presence and relevance of imagery with regard to processes of thought were as follows: Thoughts may or may not be accompanied by sensorial elements (images), and they remain unimpaired while the imagery tends to become fragmentary, obscure, and even to drop out of consciousness altogether. Images, accordingly, are not relevant to thought in the sense of being necessary to it; though they may be relevant as associated with thoughts or illustrative of them. Further, the 'universal' tends to be present to consciousness as imageless substantive content; the 'individual,' on the contrary, tends to be present as a concept in connection with sensorial contents (images). Finally, I concluded that the image best securing 'individual' thought is the direct image or percept. Possibly here, in using the term 'securing,' I was going beyond my data; for this term indicates function, and my data were mainly introspective. But even here I had the objective products of the acts of judgment made to serve as a guide to interpretation.

In the main, as far as imagery and thought are concerned, Stevanović's findings are similar to my own. The unfamiliar character of his material and the difficulty of learning it with the nonsense words, however, were calculated to be more favourable to the emergence of visual imagery when the nonsense words were presented, either for the observers to recall their 'meanings' during the learning period, or to fit predicates to them in the judgments. Ordinary meaningful words could less easily be used to designate the 'pictures' than in my own research, because the observers had no obvious words at command for this purpose. To recall any one of the particular coloured shapes exactly one would have supposed that visual images would have been even more necessary than was the case in my own research; and that Stevanović would have corroborated my finding that when we think 'individuals' we tend to have imagery of some sort. But this was not the case. Stevanović found, even in the case of observers who were 'visualizers,' that "the visual images help them very little in determining their individual meanings. The images are for most of them signs that the meanings which arise are individual; but when they came to draw the respective individual pictures they did not rely upon the image, but rather upon a knowledge of the structure and of the details of the picture in question.

In watching the observers while they were drawing the pictures, the experimenter often noticed that the execution of the drawing involved the whole series of eductions and reproductions of parts of the picture out of its relations (system, plan, symmetry, etc.). One observer... described the image he had when he obtained the individual meaning for a picture as a 'patch of green colour'; and then he proceeded to draw the whole structure of the picture with all its details. But, on the other hand, the same observer reported, *e.g.*, the meaning of an individual picture because he had a coloured image of a particular one. He further explicitly stated that the image was clear and well defined—but when he came to draw the picture, his drawing represented a merely schematic shape, which could have been taken for any picture of the group. In some cases the schematic image did not represent any particular picture of the set at all."

It will be noticed that Stevanović does not draw his conclusions merely from introspective data, but from the thought products as shown in the observers' drawings of the pictures. This is an important point, as it satisfies the criterion given above as to the method of investigating the function of images with regard to thought. Mere introspection might have suggested another conclusion, as indeed it did to most of Stevanović's observers. They, so he writes, "with one exception (a non-visualizer) tend to ascribe this 'individuality' to a meaning when they obtain a visual image, especially if a relevant image appears in consciousness prior to the appearance of the general meaning."

The foregoing citations are taken from Stevanović's notes on the learning period. He is no less explicit when he comes to deal with the way in which the 'individual' meaning is present to consciousness when the nonsense words were presented as part judgments to be completed. Here, while again generally corroborating my own results, he finds (with the single exception of judgments of the type in which the subject is indicated by an adjective denoting colour, etc.) that "the meaning of the subject in 'individual' judgments tends as frequently as in 'universal' ones to be present as a knowledge without any accompanying sensorial contents at all." This conclusion is of great interest in relation to our symposium. In the one case in which, if anywhere, imagery would seem *a priori* to be necessary for thought, it is not found in greater abundance than in any other. There need be no relevance, for there need be no image. But, when images are present in consciousness while the judgments are being made, they "serve best to indicate which individual is meant." Images answer the question 'which?' but "were not found to determine the meaning of individuals by their intrinsic characters." To answer the question 'what?' knowledge is required.

I believe these experiments of Stevanović and my own, together with those of Betts and Carey, within the range of their application, may be taken as providing suitable, and even conclusive materials for an answer to the question of relevance. Visual images are not necessary in processes of thinking such as were investigated. But, when they occur, they may very well be relevant in the sense of agreeing with, related or pertinent to, the thought going on, just as they may be illustrative of it.

I may permit myself one remark in bringing this paper to an end. Thought, in the last analysis, always has to do with things and the relations of things. If I have a visual image of a thing remembered, it

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can, as I have said, only be known to be such by being compared with that thing (percept), not now an image but thought. The thought object, or relation, must thus be independent of the image, since it is the standard by which the image is judged. This consideration, it seems to me, definitely disposes of the relevance of imagery as necessary or helpful to thought or processes of thinking. But, again, where visual imagery occurs, it may be relevant in the other senses distinguished.

Though, as I said I should be obliged to do at the beginning of my paper, I have been envisaging the wood rather than the separate trees of our problem, I trust my examination of it may have started one or two hares which will run during the discussion.

THE RELEVANCE OF VISUAL IMAGERY TO THE PROCESS OF THINKING. III

By F. C. BARTLETT.

(*From the Psychological Laboratory, University of Cambridge.*)

1. *A psychological confession* (pp. 23, 24).
2. *Definitions* (pp. 24, 25).
3. *Some criticisms* (pp. 25, 26).
4. *An outline of a general theory* (pp. 26–29).
5. *Conclusions* (p. 29).

1. *A psychological confession.*

Perhaps I had better follow the lead of the first two papers and begin by a psychological confession. In my mode of mental work I seem to stand somewhere between Prof. Pear on the one hand and Dr Aveling on the other. I am not dependent for my introspective knowledge of the characters of visual images upon what I can remember of my dreams, but I have nothing like the luxuriance of visualization which Prof. Pear enjoys. When I read the account of the tennis at Wimbledon in the opening paper I was filled with astonishment, admiration and no small amount of envy. I tried to see how I could deal with a like situation, a University cricket match. I found myself with a mass of images, predominantly visual and auditory, but other kinds as well. These were all fragmentary, condensed, of very varying degrees of clearness and vitality, and excessively disorderly in their arrangement. By whatever laws their appearance was governed these seemed to have little to do with the original sequence of events. Order, shape, form and general coherence were obtained only as in between the scatter of images came tracts of words, and thus eventually I got the whole long game back again pretty completely, with a large number of its attendant circumstances.

Maybe it is because, whether for definite recall or for processes of thinking, I normally find that visual images have to come to the help of words, and words have to connect and arrange visual images, that I am far more interested in the functional question involved in our main problem than in the question of introspective description. These questions have been very clearly differentiated by Dr Aveling, and, as he says, "introspection is necessary, but not sufficient" for the elucidation of the functional significance of the visual image.

2. *Definitions.*

Discussion in the absence of definition is more often than not discussion at cross purposes, and so I will first try to say what I mean when I use the words 'image' and 'process of thinking.' Highly controversial questions may be involved here, but I will state my view with little or no argument.

I take 'imaging' to be the reference to a concrete object or situation in the absence of peripherally aroused stimulation adequate to account for this reference. It is possible that there may always have to be *some* peripheral stimulation[1] but this is not the sole determining factor on the stimulus side of the reaction, and in many cases it cannot be regarded as the most important factor. 'Image' is the name we give to the way in which the concrete object or situation which is then referred to appears to us, when it appears in some sensory (in so far as this discussion goes, in some visual) mode.

'Thinking' is more difficult to characterize satisfactorily, because it is especially hard to frame any definition which cannot be charged with implications concerning the character of what is often called 'a thought,' whereas the definition should mainly or solely be meant to clear up how a term is going to be used. My view is practically identical with that set forth by Sir Henry Head in his recent volumes on *Aphasia and Kindred Disorders of Speech.* There are three outstanding characteristics of thinking: (*a*) it is a capacity for dealing with situations at a distance, and hence involves the *use* of signs, of which visual images are one sort; (*b*) it is a capacity for responding to the qualitative and relational features of a situation in their *general* aspect, and hence involves the *formulation* of signs; and (*c*) in the great majority of cases it is a capacity for utilizing these general qualitative and relational features in reference to a special situation, often of a concrete nature, and having a problematic significance. In other words, thinking is not merely a reference to a past situation; it is a reference to a past situation in such a way as to attempt to solve a present problem. And further, the mode of attempted solution adopted is always, in thinking proper, that of utilizing the general qualitative and relational features of the situations to which reference is more or less openly made. It is the 'problematic' character of the

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[1] Cf. G. Dawes Hicks, "The Nature of Images." This *Journal*, xv, 121–48.

thinking process which makes the notion of 'relevance' of great importance at this level of reaction. Anything is relevant to the process of thinking which can aid its attempted solution of problems. Since, as I have said already, thinking *must* use signs, and since visual images are one sort of sign, visual images may be relevant to the process of thinking. But exactly how they may be relevant still remains to be discussed.

3. *Some criticisms.*

Before I deal with this problem I must try to state some difficulties which I find in the earlier papers. It is, for me, excessively hard to understand exactly what Dr Aveling means by 'a thought,' but if I have grasped his meaning aright he and I are certainly not referring to the same sort of thing. He appears to hold that a percept may, as it were, split up into two parts, 'a copy, representation or symbol,' and something else which is veritably the thought. The symbol, or the image is something which the thought can sometimes catch on to. If, however, we refer to a situation which has originally been presented perceptually, and there is no discriminable copy, representation or symbol in image or any other introspective form, we have that situation as 'thought.' Such a view has, of course, been held by others, and I am perfectly prepared to agree that this peculiar mode of reference frequently occurs. In admitting this I am accepting the important evidence cited by Dr Aveling and also a mass of more or less similar evidence which I have myself obtained in experiments extending over a long time past. But of this mode of reference I would prefer to use the word which is most frequently employed by observers when they adopt it, and I would call it *knowing*, not thinking an object or situation. I think it indisputable that we may know that which we do not perceive, or image, or otherwise symbolize. But I cannot see any good reason for calling what we then know 'a thought.'

In his present paper Prof. Pear has left what he regards as thinking to his reader's imagination. But he was more kindly disposed in an earlier discussion[1], and said that thinking involves: (i) Recall of past experiences; (ii) Abstraction of their relevant aspects; (iii) Comparison of those experiences; (iv) Re-comparison with some aim or aims in view; (v) Combination of the results into a novel conclusion; (vi) Expression of the conclusion. Except that (vi) might be ruled out, except that all these terms name a mass of psychological processes, and except that thinking is rarely as tidily articulated as this list of its components—all exceptions which Prof. Pear would probably wish to make himself—I have little to disagree with in all this. But I cannot help wishing that in his present paper Prof. Pear had tried a little more strenuously to say where exactly in regard to all these processes, visual images come in. That they are useful in (i) everybody agrees. That they may be used in (ii), (iii), (iv) and (v) nobody denies. But we want to know just what they can do in regard to all these functions and how they can do it.

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[1] This *Journal*, XI, 72.

4. *An outline of a general theory.*

I will therefore state in outline the view of the relation of processes of imaging to processes of thinking which I wish to adopt. Imaging and thinking possess alike the general function of enabling us to deal with situations at a distance. The biological value of this is too obvious to need detailed statement. In fact, having images and being able to think are further extensions of that process of developing long-distance receptors which is at the basis of the growth of the great brain.

The next step concerns the question of how this process of utilizing situations which are no longer immediately presented is achieved. At a very lowly level indeed we have the phenomena of lowered threshold in reactions, and, perhaps at a somewhat higher level, the growth of what Head calls *schemata*. A new incoming stimulus being presented it is reacted to with a certain acquired readiness, or set into relation with preceding stimuli having the same qualitative character, or with preceding situations having the same 'instinctive' significance.

Two things are very important about this level of the organization of reactions. One is that although a present stimulus or situation is reacted to by virtue of its relation to past stimuli and situations, the stimuli and situations of the past tend to operate rather as a whole mass than as individuals. It is unnecessary to assume that any specific bit of past experience is actually taken out of its setting and reacted to as if it were more or less independent of its background. This is the type of reaction of probably about nine-tenths of our daily behaviour, mental and physiological. The other thing is that reactions, and the representations of any objects and situations towards which reactions are made, at this level tend to be organized chronologically. There is a vast amount of experimental evidence for this: chain reflexes and 'circular' reactions at a lowly level; many of the phenomena of 'conditional reflex,' rote memories, the unwinding of habits, the tendencies towards relatively fixed serial reactions which we all show when we are tired, delirious, slightly intoxicated and the like. It seems as if processes of organic adjustment are all the time striving to set up serial reactions, and as if, consciousness apart, the series is of greater weight than the elements making it up. Thus if the series fails at any part there is a tendency for complete collapse to set in or else for the whole series to begin over again.

This operation of past situations 'in the mass,' and this over-determination by chronological series are biologically unsound and uneconomical: unsound because they blur the diversity of actual environmental facts, uneconomical because to resume a whole series is often a shocking waste of time. To get out of these difficulties the method of images is evolved. I have no very definite idea as to the exact mechanism of the process; I wish I had, but in general the image is a device for picking bits out of schemes, for increasing the chance of variability in the reconstruction of past stimuli and situations, for surmounting the chronology of presentations. By the aid of the image, and particularly of the visual image—for this, like the visual sense, is the best of all our distance mechanisms of its own type—a man can take out of its setting something that happened a year ago, reinstate it with much if not all of its individuality unimpaired, compare, condense and combine it with something that happened yesterday and use them both to help him to solve a problem with which he is confronted to-day.

It will be apparent from this that I consider that most certainly the image is, in general, relevant to the process of thinking. I would in fact go further and say that in proportion as the *form* of thinking is to be given genuine material to work with so more and more must images be utilized in our thinking processes.

But to say that images are relevant in general to the thinking process is not to say that the latter is simply the utilization of the former. The device of images has its numerous defects which are the price of its peculiar excellencies. To take two of these only: the image, and in particular the visual image, is apt to go farther in the direction of the individualization of situations than is biologically useful, and the principles of the combination of images have their own peculiarities, resulting in constructions which are relatively wild, jerky, and irregular compared with the somewhat orderly march of thought.

Surely it is the first of these 'defects' that Napoleon is indicating in the saying to which Prof. Pear refers, and to nothing connected with horses, aeroplanes or even introverts. I think he means that any commander who approaches a battle with a picture before him of how such and such a fight went on such and such an occasion, or how the present fight is likely to go on this occasion, will find two minutes after the forces have joined, that something has gone awry. Then his picture is destroyed. He has nothing in reserve except another individual picture, and this too will not serve him for long. Or it may be that he has then so multifarious and pressing a collection of pictures that equally he is at a loss what adjustment of practice to make. In fact we have to go farther than the image goes. We have not only to pick out situations and events from their general setting but we have to analyse the very things which we individualize, so that we may know what parts of them may flow and alter without disturbing their general significance and function. Visualization is rather deficient in its power to do this, and words, whether imaged or not, are relatively efficient exactly in this respect.

Much more important is a study, yet to be made in a thorough manner, of the modes of combination of images of the visual kind. In reference to an original situation a visual image may be of every stage of completeness, from the most fleeting fragment to the most literal reinstatement. It is demonstrable that what determines the 'weighting' of detail within the visual image more than anything else, are those personal factors of the nature of interest or bias to which Prof. Pear refers. Thus images readily combine and condense in one mental life which to another appear remarkably ill-assorted and incoherent. The passage from one image to others in a chain frequently seems to be achieved only by a series of agile leaps. And finally the typical visualizer often seems to have such a wealth of images, and these to have such a variety of characters that he is tempted to stop and describe them—often greatly to his own and others' aesthetic enjoyment—instead of getting on with his problem.

Thinking, if I am right, is biologically subsequent to the functioning of the image-forming processes. It is possible only when a way has been found of breaking up the 'massed' influence of past stimuli and situations, only when a device has been adopted for conquering the sequential tyranny of past reactions. It utilizes the image not because the latter reinstates the specific past, but only in so far as it has bearing upon the challenge without which no effort of thinking would ever be made. This means that in thinking the qualities and relations which find illustration in innumerable images, but whose general function cannot be put into the form of any sensory image, are predominantly important. Those qualities and relations are then somehow *formulated*, not merely illustrated or represented. And that is what makes the thinking process relatively precise, orderly, perhaps somewhat unexciting, and, as compared with images, 'the same for all.'

If I may turn for one moment to the problem of the *content* of images in relation to the aim of the specific process of thinking, it is in every case the thinking itself which determines whether or not the images which are utilized are relevant, and never the images that sporadically occur which determine the relevance of the thinking they may stimulate. Probably every visualist tends to use many more images than he actually needs objectively to put his thinking through. Some of those veritably contribute that concreteness and applicability to real experience without which thinking seems apt to relapse into mere verbalization; some are by-path meadows which he had better not explore; some, apparently useless for the nonce, are really signs that beneath the dilemma which prompts his present thought he is, maybe all unconsciously, stretching out towards some wider problem. By following the last he may go into a new land, and make eventually his most bright discoveries.

5. *Conclusions.*

I have tried to state a large and speculative hypothesis in a small compass. Obviously I have left most unsaid. I have spoken as if there may be a pre-image level, an image level, and a post-image level. But life is an evolution and to talk of 'levels' is to use a device for separating things and reactions which in fact run into one another. In particular, when we pass from one 'stage' to another we take with us the acquirements of the earlier and use most of them, transforming them to meet our enlarged problems. So the chronological method is utilized in imaging, and both it and the image methods are employed in thinking. How, it is for specific research to discover. Broadly I would maintain that those levels do exist, and are as I have stated them. This is a view which has in no wise been manufactured for the present discussion, but which has grown up gradually and in the main on the basis of experiments on imaging and thinking which I have tried to carry out during the past fifteen years. It has then some facts in its support which I hope some day I may be able to state more fully than has been possible here.

DOES THE PSYCHOGALVANIC PHENOMENON INDICATE EMOTION?[1]

By R. J. BARTLETT.

I. *The Problem* (pp. 30, 31).
II. *The Experiments* (pp. 31–44).
III. *Interpretation* (pp. 44–49).
IV. *Conclusions* (pp. 49, 50).

I. The Problem.

At the International Congress of Psychology at Groningen Dr Aveling gave a concise summary of "The Conative Indications of the Psychogalvanic Phenomenon." In concluding he stressed "the necessity of further analysing conscious process" and "that conation and affection themselves should be distinguished within orectic process in general[2]." The present paper attempts a contribution to that analysis and distinction.

Argument for the conative basis of the phenomenon as given in the paper may be summarized as follows:

1. *Emotion* (awareness of somatic resonance) is not the cause, because it is "most frequently subsequent to the experience related in the relevant introspections."

2. *Affection* (pleasure-unpleasure) is not the invariable cause because it is seldom reported when there are deflections and when reported is often unaccompanied by deflection.

3. *Conation* is *a* cause, possibly *the* cause, because subjective estimates of conative effort ('alertness' and 'striving') tally with the size of deflection recorded.

On the basis of personal experience as subject and experimenter in researches on which the argument is based, I am in general agreement with these conclusions; but it is possible that 'alertness' is the end phase of a complex experience in which the earliest introspectible awareness is better classed as 'feeling[1]' than as conation. To secure convincing evidence of this has been one of my aims ever since, in the first research, in preoccupation with something I described as 'shock,' I failed clearly to isolate 'alertness.'

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[1] From a paper read before the British Psychological Society on October 16th, 1926.

[2] *VIIIth Int. Cong. Psych.* p. 232.

Emotion as the mental correlate of 'somatic resonance' is ruled out, as certainly subsequent to, and probably consequent upon, earlier mental states. This definition of emotion, however, is not as yet generally accepted and so the title of this paper remains possible: 'emotion' meaning something else. For this 'something else' that precedes 'somatic resonance' and the resulting emotional 'awareness' the word 'affection' has been used; but if its use is to be restricted to 'pleasure-unpleasure' we may be driven to discard it, too, for 'pleasure-unpleasure,' as 'alertness,' would seem to be an end state emerging from something far more chaotic and elemental, for which I can find no better word than 'feeling.'

The experimental basis of the 'emotion' theory of the phenomenon consists in using 'nocive' and other stimuli that produce marked conative activity and is therefore inconclusive. The researches on which the 'conation' theory are based are open to the criticism that, being researches on conation, they are inadequate for proof that psychogalvanic phenomena may not sometimes indicate 'emotion' rather than 'conation.'

II. THE EXPERIMENTS.

With these facts in mind we endeavoured to produce under experimental conditions mental states in which 'feeling' should dominate any accompanying cognitional or conational awareness, and have secured tachogram records of the accompanying changes in resistance of the subject's hand. A time record was secured by breaking the galvanometer light circuit every five seconds and a 'writing board,' that when depressed completed the circuit of a small light in the camera room, enabled stimulus and other times to be recorded on the photographs.

By way of introduction consider Diagram I. This consists of records from subjects *A* and *B* in experiments on conation which are more particularly reported in the Groningen paper. Mathematical tasks (P) of increasing difficulty were given verbally after a general instruction (I), the subject responding verbally (V) with the answer or by intimating that he had 'given up' the problem.

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[1] The word *feeling* is used in preference to emotion, affection or pleasure-unpleasure as more inclusive and less open to the danger of unwitting misinterpretation. We have in mind the possibility of isolating the common factor in the extremely varied experiences covered by the common usage of the word *feeling*, and have reached the tentative conclusion that this common factor is a subjective pathic state.

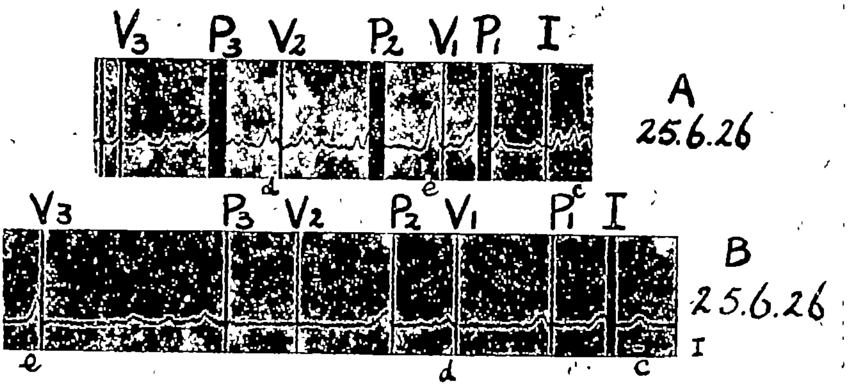

Diagram I. Problems—(Volition and Conation Experiments).[1]

The instruction and problems were as follows:

	A.	*B.*
I ...	"Solve the following equations"	"Perform the arithmetical tasks indicated"
P_1 ...	"$3x + 6 = 18$"	"Multiply 17 by 23"
P_2 ..	"$x + y = 3;\ x - y = 1$"	"Square 19"
P_3 ...	"$x^2 + 7xy + 12y^2 = 0$"	"Cube 17"

B reported that in working the tasks there was definite mental strain increasing from P_1 to P_2 to P_3, but that the experience was cognitional mainly. *A* found P_2 more difficult than P_1 and 'gave up' in P_3.

Accepting 'strain' and 'difficulty' as indicating 'striving' to perform the task, a close agreement is shown between conative activity and size and number of deflections. But both 'strain' and 'difficulty' are usually taken to include a marked 'feeling' element and the fact that *B* approached mathematical problems with more confidence (or less anxiety) than *A* is possibly reflected in the greater simplicity and steadiness of the record of *B*. With this, however, we are not particularly concerned. Rather would we stress the deflections occurring in the rest periods. Deflections *c* correspond to awareness that the experiment is about to commence, *d* to 'doubt' or 'anxiety' about correctness of answers and *e* to 'relaxation' and 'satisfaction,' in the case of *B*, at the conclusion of the whole experiment and, in the case of *A*, at securing a supposedly correct answer.

After a short rest pause, in which the camera was stopped but the subject remained in circuit, the above 'problem' tasks were followed by 'emotional thought' tasks (E). After the general instruction (I) "Dwell in your thoughts upon the following," things that possibly, probably or certainly had 'emotional' value for the subject were mentioned at intervals of from 10 to 20 seconds and the records of Diagram II were obtained.

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[1] The original records are 2 inches wide.

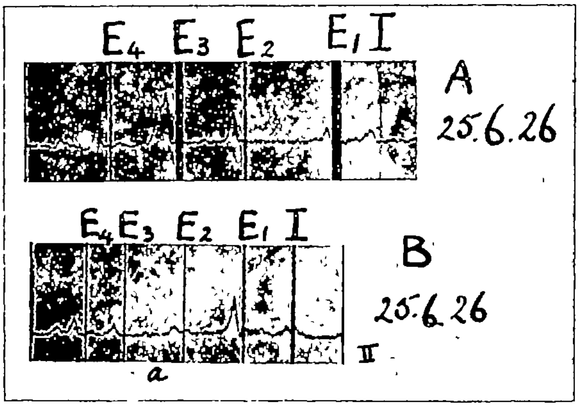

Diagram II. Emotional thoughts.

The thought suggestions were as follows:

		A.	*B.*
E_1	...	"An April shower"	"Snakes"
E_2	...	A place name possibly having emotional value for *A*	"Your favourite hymn"
E_3	...	A name known to have high emotional value for *A*	"A garden of roses"
E_4	...	A name known to have emotional value for *A*	"The smell of a corpse"

At the close of the experiment *A* reported that E_1 had no emotional value, that E_3 gave highly emotional associations *on the cognitional plane* and that the associations of E_4 and in lesser degree E_2 were also emotional. The present experiences, however, were classed as 'almost entirely cognitional.'

Similarly *B* reported that E_1 was predominantly cognitional but that memories arising had high emotional value originally, that in E_2 he failed to reach a definite 'favourite' and that again the experience was mainly cognitional but linked with matters of a highly emotional nature; that E_3 was distinctly pleasant with an added enquiry as to why particular gardens came into memory; and that in E_4 the suggested task was evaded and then gave the 'dogfish in zoo. lab.', the experience being dominantly cognitive with a vague awareness of evasion of emotional material.

In *A* the expected and reported order of emotionality of aroused memories is reflected in the size of deflection and, while the other stimuli produce simple curves comparable to those produced in practised subjects by a single sharp clap of the hands, that having the highest emotional value (E_3) gives a complex curve which is followed by a rise in resistance sufficiently rapid to give a well-marked positive deflection. This rise was still in progress when E_4 was given, and, after the clear cut fall for E_4, continued at a still greater rate. The associations of E_3 were unpleasant while those of E_4 were pleasant.

Subject *B* when attempting to arrange the experiences in order of emotional value reported that the pleasure-unpleasure was memory of past experience rather than present experience. No. 3 approached most closely to a present pleasure. No. 1, after initial shock, on a vaguely sinister background, gave a complex of several pleasant memories. No. 4 produced awareness of vague possibilities that were at once evaded. Had the possibilities developed this would have been the most emotional (unpleasant), but the revival of the dogfish with its smell and interest (it was being dissected) was slightly pleasant. With No. 2 there was a slight feeling of unpleasure when it was recognized that no clear favourite had arisen. Omitting 4, the order of emotionality of the memories was given as 1, 3, 2. If the inhibited (?) memories of 4 are assessed it ranks as equal to or greater than 1, but if the substitution is assessed it equals 2.

The markedly cognitional nature of the experiences led to attempts to secure present emotion as opposed to memory of emotional experience. In the first of these a series of tasks was given, designed to secure the record of a present emotional experience in a series of records of cognitional and conational experiences.

The records sought were those accompanying (1) Mental response to a threat, (2) Silent counting, (3) Counting aloud, (4) Verbal report, (5) Memory of an important decision, (6) Memory of a highly emotional incident, (7) Mental response to suggestion of a concrete 'emotional' incident, (8) Written report.

The Tachogram record of this series is given in Diagram III.

When the subject (*A*) had been in circuit for three minutes the experimenter (*E*) said "Remain perfectly still until I return." *E* then left *A* and went to the camera room and at B started the camera, and walked back to *A*, the deflection *b* probably corresponding to his entry into the subject's room.

At C, *E* said: "I propose to give you five minutes' torture." After the close of the experiment *A* reported that there was "active thought of what the torture would be." Allowing for the latent period, there is a small deflection, *c*, at the commencement of the 'instruction' and a large one, *d*, at its termination, *i.e.* on cognition of the key word 'torture.' We are inclined to associate the 'active thinking' with the smaller movements[1], *e–e*, that follow the big throw of the galvanometer mirror.

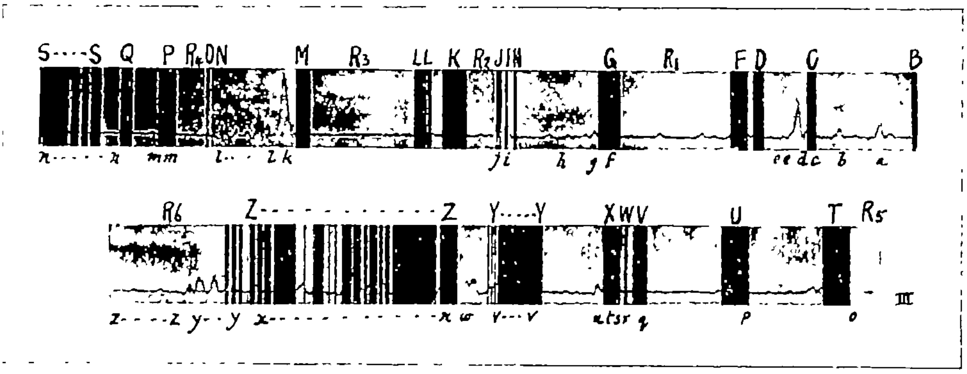

Diagram III. Subject *A*. 7. 7. 26.

At D, *E* gave the general instruction "Obey instructions as fully and carefully as possible" and then, at F, added "In particular, answer questions quickly and concisely."

After a rest pause, R_1, of 30 seconds, the first task was given at G in the words "Count mentally as rapidly as possible from one upwards until stopped." About 25 seconds later, at H, *E* said "Stop!" and at I added "What was your last number?" *A* immediately replied "116" and *E* recorded the answer in writing at J and then, after a short rest pause, R_2, at K, *E* said "Evaluate conation of counting experience—Good, Medium, Poor." Quickly *A* responded "Fair—with two hitches." *E* recorded this at LL and then gave a rest period, R_3, of 30 seconds. The large deflection at *f* would seem to correspond to apprehension of the task: the break in the upward curve possibly corresponding to the apprehension of 'rapidly' as a factor increasing the difficulty of the task. The smaller deflections, at *g*, correspond to acceptance of task and commencement of counting. There is no way of fixing exactly when the reported 'hitches' took place, but from consideration of other records we are disposed to associate the rapid movements, *h*, including rises as well as falls in resistance with the experienced difficulty of dealing with the task. A similar record, though not so pronounced, can be seen in Diagram II *B*, where *a* is probably associated with awareness that the task was not being performed and a flustered unsuccessful attempt to secure the favourite. The deflections *ij*, corresponding to H and I respectively, are the ordinary type of deflection secured by simple 'nocive' and 'significant' stimuli. It should be noted that the response had been given and was being recorded by *E* before the second deflection had reached its maximum. The short rest R_2, instruction K, and verbal reply recorded at LL, give but small deflections and are followed by the very smooth rest period R_3.

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[1] Simultaneous records demonstrate that tachogram records of this nature correspond to a marked fall of resistance as measured by the galvanogram.

The next task was then given at M in the words "Count aloud as rapidly as possible from one upwards until stopped." Counting continued for 25 seconds, then, at N, *E* said 'Stop!' and, at O, recorded the last number, '140.' After a rest, R_4, of about ten seconds, *E* said at P: "Compare conation of loud with silent counting—Greater, Equal, Less?" *A* paused for a short while and then gave a reply from which the principal words 'Difficult...Probably less' were recorded by *E* in writing at Q. A rest was to have followed here, but *A* had not finished and proceeded to give a report in explanation of his reply. From this report *E* secured as key words and recorded at SS "Physical concomitants...effort greater in silent." *A* stressed the great difference in the conditions and the fact that awareness of movement of teeth, etc., was very marked in the loud counting, but expressed strong conviction that mental effort was decidedly greater in the silent counting. Apart from slight deflections, *mm*, at task P the only noticeable deflections are during the actual counting period. These show rapid rises as well as falls in resistance and in some respects the record may be looked upon as a large scale variant of the rapid movement records corresponding to awareness of 'hitches' and 'failure' referred to above. The increased size of the deflections is not a correlate of introspected increased conation, for *A* was convinced that the mental effort was greater in the silent counting. In view of the straight line record, *nn*, accompanying a verbal record of approximately equal duration given at a fairly rapid speed, it is unlikely that speech movements alone are responsible for the deflections. The experimenter noted that during the vocal counting *A* "stumbled several times and had difficulty in starting again," and we are disposed to associate the 'jumpy' record with the feeling accompaniment of awareness of possible failure and breakdown of the effort rather than with the actual effort. It is more disconcerting to break down before an audience than in the privacy of one's own mental awareness. Possibly a distinction should be drawn between the initial large deflection, *k*, and the later deflections, *ll*; *k* being the result of apprehension of the task and its commencement, *ll* the result of anxiety resulting from incipient failure.

After *A* had stopped speaking and *E* had finished writing, a further short rest pause, R_5, preceded the next task. It will be noticed that, allowing for the latent period, the mental correlate of a series of deflections, *o*, occurred some three or four seconds before *E* began to give the task. It seems probable that this corresponds to expectation that *E* was about to take the next step.

In the next two tasks, which we will consider together, attempts were made to evoke vivid memories of (1) a conative and (2) an emotive experience, in the hope that it might be possible to compare 'cold reason' with 'unreasoning emotion.' In this we failed, for the dominant feature of the conative experience was the accompanying emotion and the same experience later did duty as the emotive experience. At T, *E* said, "Secure and dwell upon the memory of making some important decision" and at U "Stop! Secure and dwell upon the memory of some highly emotional incident in your life." In the last task of the sitting, *A* wrote, with reference to each of these tasks, "Most emotional—but mere memory and no present emotion." Later he stated that the same incident did duty in both tasks. Once again the attempt to secure introspective evidence of 'present emotion' under experimental conditions has failed; but we may note that the second revival is accompanied by smaller deflections than the first and that the small deflection *p* apparently corresponds to apprehension of the noci-significant stimulus 'Stop!'

We come now to the most important task in the series. In it an attempt was made to bring together memories of two matters of known emotional value and to suggest a line of thought that probably had not been previously worked out by *A*. It was hoped that this would produce a present state that would be definitely recognized as highly emotional and the actual instruction was of such a nature that *E* afterwards reported that he wished the electrodes had been on his own hands for, for him, the grave uncertainty as to what might happen produced a highly emotional situation. We wish so, too, for at the end *A* made the written record "on notional plane" and added two words which were present as inner speech and apparently correspond with deflection *u* and, for the subject, an immediate successful treatment of the task.

At V, *E* said "Stop! Consider carefully the probable mental state" [slight pause and then at W] "of..." *a key word* calculated to call up a group of emotional incidents [another slight pause and then at X] reference first to *A* and then to the emotional incidents at the basis of stimulus 3, in record *A* of Diagram II, including a *second key word,* and finally, in the closing words, a disclosure of the concrete task. In explanation of the written record, after the sitting was finished, *A* stressed the fact that there was 'no actual emotion' and questioned whether this might not be an instance of 'inhibition.' We think it possible that the solution found was of the nature of a substitute such as the 'dogfish' of case 4, record *B*, Diagram II, and that *A* successfully avoided definite consideration of the suggested concrete incident; but the large complex deflection, *rst,* of over five seconds' duration calls for explanation. The deflection took place while the latter half of the instruction was being given and, allowing for the latent period, crest *r* corresponds to apprehension of the first key word, crest *s* to the pronoun 'you' and the double crest, *t,* to the second key word. Apprehension of the task in its entirety evoked an inner speech response that gave a satisfactory termination to the state of tension, but during the comparatively slow unfolding of the task the mental state of *A,* though not reported, was probably one of suspense intensified by the knowledge that highly emotional material was, as it were, 'in the offing.' Such a state common experience finds to be dominantly emotional. We will return later to consideration of a possible reason why such an apprehensive emotional state was not reported.

At YY, *E* said "Stop! For your own information make short notes. [Slight pause.] That done, signal three distinct taps [pause] one, [pause] two, [pause] three[1], and remain perfectly still until cut out." *E* then left *A* and went to the camera room to watch the record of the writing process ZZ and allow *A* to come to a resting state before cutting him out. During the instruction YY, there is a series of deflections *vv,* which may correspond to a renewed awareness of the highly emotional nature of the 'inhibited' memories. As *E* left the room and *A* prepared to carry out the instruction to 'make short notes,' a large compound deflection *w,* extending over ten seconds of time, was recorded and during the writing period a series of deflections *xx* are shown. That these latter are not due to the mere writing process is shown by such writing records as *ww* in Diagram IV.

Some other 'cause' must be found and we think it will eventually be found to be closely linked up with the emotional nature of the revived incidents that were reported as apprehended 'on notional plane' with 'no present emotion.'

[1] See lines corresponding to three taps at end of YY.

At the close of the experiment there are four well-marked deflections *yy* before *A* falls into the resting state, *zz*. These probably correspond to relaxation after the tension of the task, although the compound nature of the third and the fact that we have four deflections when one or two are usual suggest that there was a mental return, in a questioning attitude, to some phase of the recent experiences.

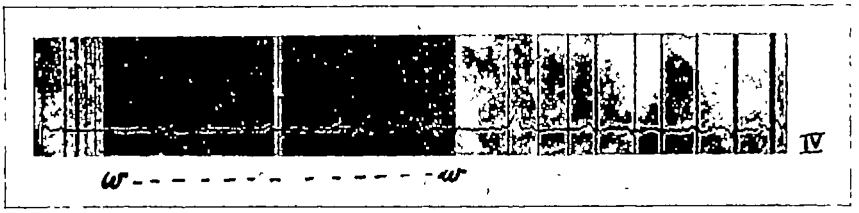

Diagram IV. Writing record. Subject *A*. 13. 9. 26.

In the above we have sought to show that explanation of the deflections by reference to 'emotion' rather than to 'conation' is at least possible, but we are faced with three difficulties when we carefully consider the subjects' reports. First there is the emphatic statement that there was no present emotion. Next there is the constantly repeated report that the experiences were 'cognitional only,' 'mere memory' or 'on notional plane.' Lastly there is the reference to 'inhibition.'

The reference to 'inhibition' may be simply an attempted explanation in terms of the theory that this reflex is associated with attempts to suppress the outward signs of emotion, or it may be the result of awareness of something in the experience that, in the light of previous experience, is interpreted as meaning the approach to clear consciousness of thoughts we would rather avoid under existing conditions. That 'something in the experience' is extremely difficult to define but from our own experiences under experimental and other conditions we would suggest that it is a vague uneasiness and uncertainty developing into awareness of the near possibility of grave anxiety, self-depreciation, confusion or sadness and is, in the current use of the term, a highly emotional state.

The other two difficulties are really one, expressed in two ways. We have again failed to secure introspective evidence of 'emotional' states under experimental conditions. The foundation reason for this seems to be that 'mere memory' of the past 'on the notional plane' has been the dominant introspectible feature of the present experience. We therefore now sought means of reducing the importance of the memory factor, decided to attempt to reproduce the states of feeling that accompany the study of works of art and commenced with postcard photographic reproductions of pictures in the National Gallery mixed with picture postcards of 'amusing' and other subjects.

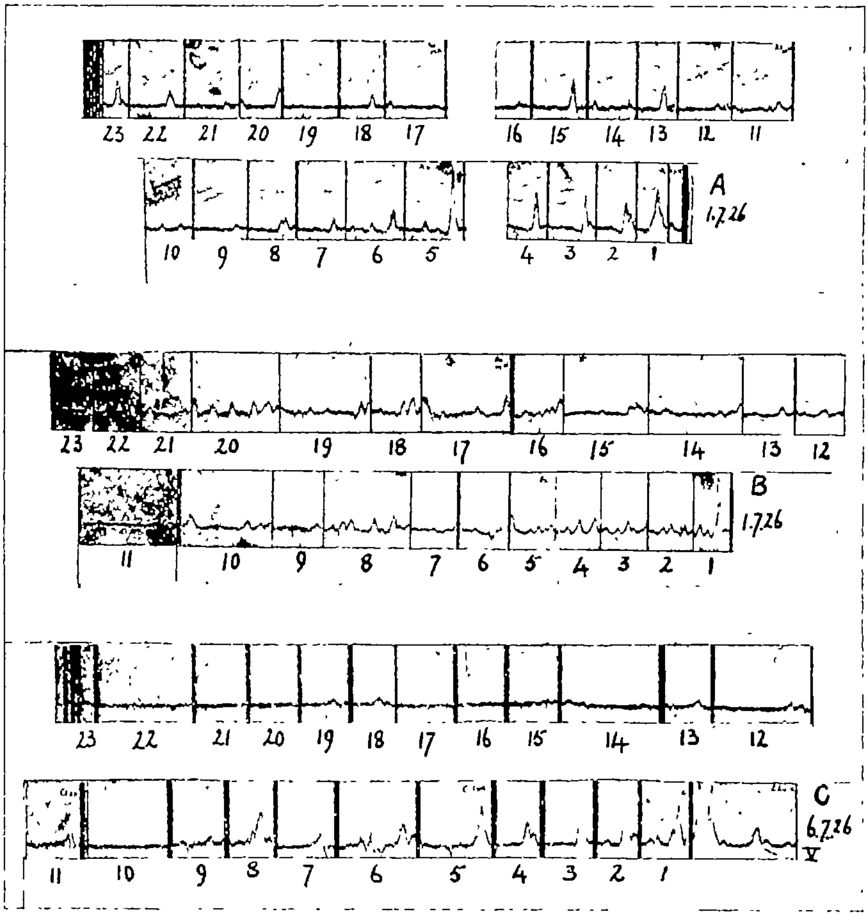

Diagram V. Postcards. Set I.

In set 1 there were 23 cards of which the first four and numbers 16 and 23 were blanks. They were placed on the writing board face down and the subject in turning them over pressed down the board and so recorded the time of turning. Records obtained from three subjects are given in Diagram V.

The number and position of the blanks were known to *B*. The records of *A* and *C* from blanks are of a similar nature and *C* reported that the number of blanks at the beginning caused considerable surprise as he expected one only. In the other records there can be traced a general similarity in the three subjects and on several occasions when one record differs markedly from the other two the difference is explained by the introspective records. Thus with no. 9—Rossetti: Fazio's Mistress (3055)—*C* reported 'admiration of beauty' but *A* and *B* simply recognized it as the work of Rossetti. Similarly with no. 17—Arthur Moreland: Oliver Twist and Fagin (a coloured postcard)—*B* "Endeavoured *to live into*, first the Jew and then the boy" and was impressed by the fear on the boy's face, but *A* and *C* passed it as "another Dickens card." There are marked deflections, as in 6, 8 and 18, when 'amusement' is reported and several other marked deflections are given by cards that have emotional value for the subject; but pleasure or unpleasure was seldom reported and again the reports stressed the dominant cognitional nature of the experiences.

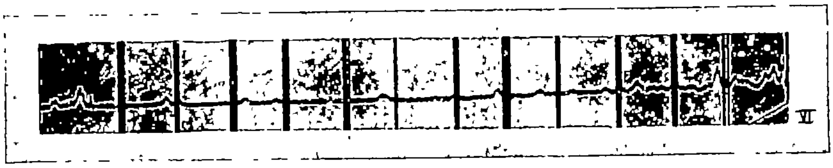

Diagram VI. Postcards, Set II. *E* turning cards. Subject *B*. 2. 7. 26.

The position of the deflections indicates that they are not due to the action of turning over the cards, but to dispel any doubt there might be on this point an experiment was made with subject *B* sitting perfectly still while the experimenter turned over a second set of postcards. The record of this experiment is given in Diagram VI.

Three months later certain of the subjects were tested with Set I a second time and it was found that their records bore a close resemblance to their own first records. In addition to the common factors demonstrated by records such as Diagram V we find individual peculiarities reproduced by the same stimulus at different times.

The deflections are the result of the mental state produced by the particular picture seen and, apart from those given by the blanks, vary roughly with the emotional value of the picture. With our practised subjects, however, present emotion was very rarely reported and it was felt that possibly larger reproductions might have a more decided effect. We therefore next carried out a series of experiments using Cassell's bound volumes, *The Nation's Pictures*, as material.

The subjects turned over the pages of the books at places marked by inserted numbered cards. They were instructed to allow each picture in turn to have its full effect on them and then to assess its pleasure-unpleasure value for them in a scale of seven grades from + 3 to − 3. When they had finished this part of the work they were cut out from the tachogram circuit, looked through the pictures again and made brief notes on their recent experiences when in the circuit.

The following two cuttings from records will show the nature of the results obtained.

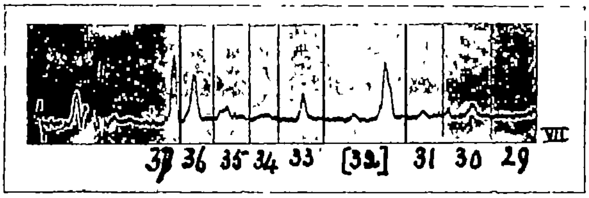

Diagram VII. *The Nation's Pictures*, vol. IV, nos. 29–37, horizontal. Subject *A*. 17. 9. 26.

* Pictures arranged in order of size of maximum throw of deflection (= highest rate of fall of resistance)	Pleasure-unpleasure estimate	Extracts from reports
37. Blank—the end inside cover	0	Astonishment
[32]. The Knight Errant—Sir J. E. Millais ...	?	Astonishment
36. Landscape—Henry Bright	0	—
33. The Remorse of Judas—Edward Armitage	− 1	Unpleasant (hate)
35. The Bonxie, Shetland—J. C. Hook	1	Pleasure?
30. A Chat Round the Brasero—John Phillip ...	0	Did not understand
31. Disturbed—J. W. Oakes	1	? Obj. pleasant
29. Abandoned—Adolphe Schreyer	− 2	Unpleasure (? memory)
34. Thursday—William Dendy Sadler	0	Familiar—stupid

* The picture *without its title* was seen.

There is no correlation between the estimates and deflections, but 'astonishment' and 'unpleasant (hate)' head the list. With reference to the 'astonishment' at the final blank, on another occasion the same subject reported that he was "thwarted—wondered, and then realised." On both occasions, however, he stressed the absence of 'emotion.' No. [32] was seen in error—a vertical picture instead of a horizontal one. The full record reads: "Recognized position of picture—many thoughts of other things connected with it. Start and astonishment that there was no card, etc."

Consider next the portion of a record from *B* given in Diagram VIII.

Here we have three well-defined deflections, at nos. 4, 7 and 8. The three pictures are "Two Strings to her Bow"—John Pettie; "The Expulsion from Eden"—Arthur T. Nowell; "Maiwand: Saving the Guns"—R. Caton Woodville. In the first case the subject was "captivated by the impishness of the woman's smile," in the second he reported "admiration of beauty," and in the third "expansiveness...feeling of success." The other three pictures "Off Valparaiso," "Ruth and Naomi" and "August Blue" brought memories of past pleasure reported as 'cognitional.' Again there was no correlation between speed of fall of resistance and estimated pleasure-unpleasure.

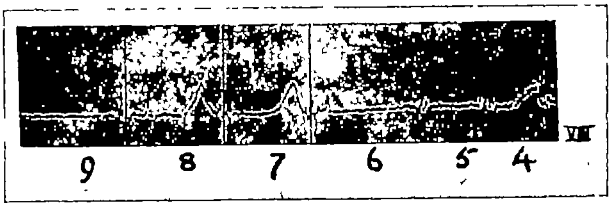

Diagram VIII. *The Nation's Pictures*, vol. I, nos. 4–9, horizontal. Subject *B*. 17. 9. 26.

It is possible that the instruction to assess pleasure-unpleasure is not a satisfactory one. Admiration, expansiveness, astonishment, captivation would all seem to include a marked feeling element that cannot readily be measured in terms of pleasure-unpleasure. The subjects were therefore next asked to attempt to assess the feeling value of experiences described by such words without undue stress on the pleasure-unpleasure component of the complex. Under these conditions indications of definite correlation between the estimates and records have been obtained. In one case, subject *B*, with 24 pictures that he had not seen under the experimental conditions before, gave estimates that tally with a classification of the accompanying deflections into four grades as in the following table.

TABLE. *Subjective Estimates of Feeling and Recorded Deflections.*

Subject's estimates + or −	Grade of deflections			
	3	2	1	0
3	2	1		
2	1	5	2	
1		3	5	2
0			1	2

It would appear from this table that the subject has been able to estimate *something* in his experience, variations in which are accompanied by variations in the size of the tachogram deflections. That *something*, however, is not simple 'pleasure-unpleasure' but a variety of experiences more generally classified as complex emotions. Professor McDougall finds in 'wonder' and 'negative self-feeling' a complete analysis of 'admiration.' 'Astonishment' in the records is associated with 'wonder,' while the 'thwarted' feeling in the complex is perhaps a compound of incipient 'anger' and 'fear.' 'Expansiveness,' which has been reported several times by one subject and correlates highly with tachogram deflections, would seem to contain 'positive self-feeling.' Then we have the termination of experiments often followed by a deflection that goes with reports of 'relaxation' or 'satisfaction' on supposed satisfactory performance of the task and by deflections, apparently of a different form, when 'doubt,' 'worry' and such like are reported on the supposition of failure. 'Nervousness,' 'flurried helplessness,' 'doubt' are reported, with deflections, when the task in progress is found to be difficult but is not definitely 'given up'; and similarly 'apprehension' and 'trepidation' when the unknown character of the pending task looms large.

Many, perhaps all, of the states that the subjects are endeavouring to report contain an element of striving that is introspectible and if this element be unduly stressed all may perhaps be classed under conation. We prefer, however, the conclusion that within orectic process in general it is necessary to distinguish between conation and affection and suggest that, in most of the states our subjects are attempting to record, conation is not the dominant element.

III. INTERPRETATION.

Definition by example is a favourite device when dealing with conation. Among given examples are: interest, craving, longing, yearning, endeavour, desire, purpose, wish, will, appetition, aversion, impulse, consent, satisfaction, disappointment[1]. Do such definitions, however, mean more than that conation, as opposed to cognition, is a factor in the mental states enumerated? We doubt if anyone can be found who would argue that both cognition and feeling are absent, and submit that in interest, craving, desire, satisfaction and disappointment, *feeling* rather than *doing* is the dominant factor, while even in endeavour and purpose, and more so in appetition, aversion and impulse, *feeling* is far from negligible.

Locke in stressing the difference between 'desire' and 'will' finds that

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[1] *Vide* Stout, *Manual*, p. 64, and Spearman, *Nature of 'Intelligence,'* p. 53.

"the uneasiness of desire [and the other 'passions' or 'affections'] determines the will[1]." Professor McDougall's treatment of the relation of 'emotion' and 'volition' would seem to be similar[2]. For both the mental correlate of activity is conditioned by the preceding state in which the subject is dominantly 'passive' or 'suffering.' Professor Stout in discussing conation speaks of "an inherent tendency of conscious states to become something different[3]." If we stress 'conscious state' we arrive at rapid change from static event to static event: if we stress 'becoming' we have dynamic evolution of an experience extended in time. The 'something different' becomes eventually an 'end-state' and on the way may be many other 'things.' Too often in psychological discussion the sections of experience considered are transverse sections when longitudinal sections would be more illuminating; we stress simultaneity when an essential of the problem is the fact of succession. Change this, and then, instead of classing interest, craving, longing, etc., together as cognate terms for the same, or similar, mental events, we secure successive phases in an extended experience—as interest, desire, decision—in which from dominant felt passivity at one end we pass to dominant purposive activity at the other.

During the passage we have a blend, in constantly changing proportion, of 'action' and 'passion,' 'striving' and 'uneasiness.' In present day usage an intermediate stage, with 'striving' stressed as the dominant component, would appear to be a typically conative state. That such usage leads in our 'problem' experiments to the necessity of drawing a clear distinction between conation and volition[4] in the form of simple 'will act' indicates that present usage is open to criticism. Under certain conditions at one end of a changing conative mental state we find 'emotion' and at the other 'volition.' Viewed from this angle 'volition' has little greater claim to be classed as conation than has 'emotion.' That the one decidedly is, and the other, just as decidedly, is not, classed as conation is the result of stressing the end phases rather than the beginning phases of the complete experience.

The typical 'conative state' of present day usage is a complex state in which 'action' and 'passion' are blended but 'passion' has been ignored; moreover the state is not a 'static event' but a 'dynamic experience.' The mental states commonly catalogued as examples of conation are phases of complex 'orectic process' within which conation and feeling are blended. When feeling as 'affection' is dominant we have 'emotion'; when conation as 'willing' is dominant we have 'volition'; when conation as 'striving' is dominant we have typical 'conation' of present usage. It has been shown that, on the whole, tachogram deflections are larger with typically 'striving' experience than with typically simple 'will-act' experience. We would find the explanation in terms of the feeling component of orectic process.

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[1] *Human Understanding*, Bk II, chap. XXI.

[2] *Introduction to Social Psychology*, chaps. VIII, IX.

[3] *Manual*, p. 64.

[4] *VIIIth Int. Cong. Psych.*, Aveling, p. 232.

Consider next the claim of conation in the form of 'alertness' to be the mental correlate of the psychogalvanic deflections. After quoting Dr Thouless as saying "The condition of low resistance is apparently a condition of preparedness to react to a stimulus," Aveling adds "The *significance* of the stimulus...brings about an 'alertness' or preparedness for reaction[1]." 'Alertness' or 'preparedness' is the correlate of the 'low resistance.' It is thus, as we said earlier, an end state. The essential part of the psychogalvanic phenomenon, however, is not so much the resulting lower resistance as the *fall* in resistance—which, alone, is recorded in the tachogram deflection. This fall in resistance in a typical case of 'alertness' corresponds to the change from the 'stand easy' resting position of general casual readiness for future events to the preparedness for immediate purposive activity that follows an equivalent of the verbal command: 'Attention!' The mental correlate of the beginning rather than of the end of this period is significant when we seek the mental cause of the fall in resistance. We would characterize the beginning state as one of 'unpreparedness' rather than of 'preparedness' and argue that the fall in resistance corresponds to the change from unpreparedness towards one of preparedness.

"The *significance* of the stimulus brings about alertness," but before that significance has been apprehended there is an earlier phase in which that which was occupying the mind drops out as a *new*, at present unknown, *something* breaks in. It is this initial phase that, partly as a result of previous experience in 'free association' work, kept me fully occupied when a subject in the first research of the series on conation and which I then described as 'shock.' With repetition the readiness of 'alertness' is secured more and more easily, with less and less 'shock' and with smaller and smaller deflections. But if, in the waiting period, the practised subject forgets the experiment that is in progress and becomes occupied mentally with something else, or if, due to some error of the experimenter, failure in the apparatus or other cause, something 'unexpected' happens, marked 'shock' experiences result and are accompanied by large deflections. As examples of such deflections we would note the deflections at the unexpected blanks in Diagram V and at 37 and [32] in Diagram VII.

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[1] *Ibid.* p. 231.

'Alertness,' 'preparedness,' 'vigilance' under certain conditions are suitable names for the end state of a complex experience that commences as the 'shock,' 'start,' 'surprise' or 'astonishment' of our subjects' reports and the 'choc' of Wechsler[1] if we interpret his physiological definition in terms of mental correlate. But the end state is not necessarily one of alertness. When it is recognized that the stimulus creating the shock has no further significance in the present situation, the subject, instead of becoming 'alert,' drops into the resting position. The fall in resistance goes with the 'shock,' and when this speedily passes into satisfactory awareness of the nature of its cause, and whether the satisfactory awareness takes the form of 'alertness' or of 'relaxation,' the tachogram record is a simple bell-shaped curve such as the initial deflections in tasks 1, 2 and 4 of record *A* in Diagram II. This is the typical case in the conation researches and, normally, so speedy is the passage to satisfactory awareness of the significance of the stimulus that it is not surprising that for most subjects the dominant introspectible component of the experience is what was finally called 'alertness.'

When, however, the initial phase is not quickly followed by satisfactory awareness of the nature of the stimulus the process of 'becoming alert' is drawn out and the subject passes through a mental state in which it would seem the 'uneasiness' of Locke is the dominant component. Our subjects described it as 'apprehension,' 'trepidation,' 'incipient fear,' 'nervousness,' 'flurried helplessness' and 'feeling creepy all over.' With subjects accustomed to the technique of the experiment, such reports are accompanied by complex deflections, such as deflection *rstu* in Diagram III. Better illustrations, however, can be given from the initial records of those unpractised subjects who for one reason or another approached the experiments with a certain amount of fear. It was one such who in describing the state of fear that developed on being left alone in the subject's room gave the description 'feeling creepy all over.'

Somewhat similar records, though on the whole showing less violent changes, are obtained when 'doubt,' 'worry' or 'anxiety' are reported; but such reports are also accompanied by records showing the small rapid changes dealt with earlier. Pleasant emotional experiences such as 'admiration' and 'expansiveness' are accompanied by long-lasting fairly smooth curves such as 7 and 8 in Diagram VIII, and with 'satisfaction' and 'relaxation' at the termination of a task or series of tasks are found simple bell-shaped curves of similar nature to the 'shock-alertness' curves.

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[1] "What Constitutes an Emotion?" *Psychological Review*, 1925, p. 235.

'Shock,' 'uneasiness' and 'relaxation' as experienced by our subjects would seem to be fundamentally subjective states in which objective reference, though probably never entirely absent, is small in amount and extremely vague in quality. The purely subjective state, if obtainable, is short-lived and continually passes by objective reference into cognition or conation. With clearer cognition of the cause of the mental state, subjective 'uneasiness' gives place to unpleasure or pleasure accompanied by aversion or appetition and a complex emotion, in the sense in which McDougall uses the term, develops. Reports such as 'admiration' and 'expansiveness' belong here and are attempts to pinion with a name states in which 'passion' and 'action' are almost inextricably entwined, but in which the dominant factor—and the reason for the presence of the other factor—is 'passion.'

We are now in a position to suggest a possible explanation of the great difficulty experienced in our attempts to secure unequivocal introspective records of present emotional experience. Introspection, accompanied as it is by intention to give, if possible, an intelligible report, involves objectification. Objectification appears to demand external reference and 'cognition' becomes an ever present component of introspected experience in which the subjective factor, if present in awareness, is tacitly ignored. With conation, too, there is this external reference, but according to many workers in this field it is accompanied by an internal reference which in its clearest form is a definite 'awareness of self.' With pure 'feeling' there is no external reference at all. The whole experience from the introspective standpoint is made up of internal reference. It is a passive state of the self including no reference whatsoever to external causes. The instant external reference enters into the state we have cognition of possible causes of the feeling and conation with reference to those causes. Thus the state of pure feeling quickly passes into cognitive or conative states and it may well be questioned whether the completely 'passive' or 'suffering' state is not an unattained limit towards which introspectible states such as that we have described as 'shock,' approximate.

It may be that introspection of pure 'feeling,' in this sense of subjectivity unalloyed, is impossible, but that in some way *after* living through an experience that to introspection yields nothing but cognition we become aware that it contained a phase in which 'feeling' was dominant. This would seem to be the conclusion of Professor Spearman[1].

This inability introspectively to apprehend 'purely subjective' states might be used as an argument against their existence. Rather would we argue the limitations of introspection, agree that "subjectivity must be taken as a primary character, and objectivity as only something superposed[2]," see in cognition and conation products of introspective objectification rather than actual apprehended experience and find in pure feeling the ultimate basis of all awareness.

Ward, in his general analysis of experience, reaches a succession in which attention to sensory objects (*cognition*) results in *feeling* (pleasure-unpleasure) which, in turn, passes over into attention to motor objects (*conation*)[3]. Earlier, however, he gives 'feeling' first place when characterising the elementary facts of experience as "I feel somehow," "I know something," "I do something[4]." We suggest that from a purely subjective "I feel somehow," all else is secured by subjective activity: which for Ward is 'attention' and for Spearman is 'apprehension' or 'eduction.' Stress subjective activity and the 'conation' theory of the 'reflex' emerges. Stress the dependence of activity on pathic feeling that when stripped of all external reference is liveable, but not in the strict sense of the word introspectible, and the 'emotion' theory becomes the more satisfactory.

IV. CONCLUSIONS.

The psychogalvanic phenomenon follows a variety of complex changing mental states many of which would ordinarily be described as 'emotional' and yet may yield to introspective enquiry cognition and/or conation only. It seems best in the present state of our knowledge to describe these complex states as 'orectic process' in which 'feeling' and 'striving' are mingled; but in view of the fact that when cognized, the initial phase of markedly conative experience, accompanied by deflection, is reported to be dominantly subjective in character, and that large deflections are obtained when the dominant awareness is one of being impressed by the cognized stimulus, it seems probable that the mental cause of the physiological change that brings about the deflection is of the nature of passive endurance or enjoyment rather than of active striving or willing.

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[1] *The Nature of 'Intelligence,'* p. 228.

[2] *Ibid.* p. 229.

[3] *Psychological Principles*, p. 56.

[4] *Ibid.* p. 34.

The tendency of the purely passive state to pass into and be lost in an active one makes the search for crucial tests under experimental conditions a difficult one, but our work seems to indicate that the shape of the tachogram curve varies with the nature of the differing mental states that immediately precede the 'reflex' and that among distinguishable occasions on which the phenomenon appears are the following:

1. When something not consciously expected and, at first, inadequately apprehended enters awareness. This is the 'shock' experience of our experiments and covers as sub-groups the 'nocive' stimuli experiments of earlier workers and the 'alertness' cases of the 'conation' theory.
2. When a task is concluded with satisfaction or relief: the 'relaxation' of our experiments.
3. When 'doubt,' 'anxiety,' 'fear,' 'apprehension' intervene: the 'uneasiness' of Locke, the 'striving' cases of the 'conation' theory and, possibly, the 'complex indicating' cases of medical psychology.
4. When states described in such terms as 'admiration,' 'expansiveness' intervene: the 'complex emotions' of McDougall.

A simple formula to cover these four cases presents difficulty. There is a common 'feeling' element but that in itself is possibly insufficient explanation of the physiological change recorded by the galvanometer. If we are right in looking to *complex* orectic process for the understanding of the phenomena rather than to 'conation' or 'emotion' alone, we must seek a common conative element and when we seek it one or other of the cases seems always to stand outside. The changing activity of 'striving' is not a common factor. 'Enjoying' or 'enduring' rather than 'striving' would seem to describe the active element in 2 and 4. The waiting attitude of 'alertness' is not a common factor. Possibly the expectant element in 'alertness' is the factor we seek; but case 2 still seems exceptional.

We need on the conative side the equivalent of the purely subjective pathic state that we have argued is the common factor in all feeling. Possibly that equivalent can best be described as a change in attentional attitude. Possibly the ultimate mental basis of the phenomenon is a change from non-voluntary awareness of, to voluntary attention to, a pathic state of the self, coupled, by speedy 'external reference,' with a becoming aware of a possibly 'significant' change in, or differentiation of, the 'presentational continuum.'

CHILDREN'S APPRECIATION OF POEMS

BY LYNETTE FEASEY.

I. *Purpose and Method of the Experiment.*
II. *Children's Preferences.*
III. *Classification of Children's Reasons.*
IV. *Discussion of the Different Types of Judgments:*
  (1) *Judgments referring to subject-matter.*
  (2) *Judgments referring to language and rhythm.*
  (3) *Subjective judgments.*
  (4) *Character judgments.*
  (5) *Judgments referring to the author.*
  (6) *Judgments based on imagery.*
  (7) *Associative judgments.*
  (8) *Objective judgments.*
  (9) *Pragmatic judgments.*
V. *Individual Differences.*
VI. *Some Educational Conclusions.*

I. PURPOSE AND METHOD OF THE EXPERIMENT.

THE aim of the following experiment was to gain some light on the attitudes which children adopt to the poems they study in school. The experiment was carried out in the girls' departments of two Leeds elementary schools; 13 classes were used, ranging from Standard I to Standard VII, making a total of 550 children.

The class teacher wrote on the blackboard the names of six poems recently studied by her with the class, and the children were asked to say which of these poems they liked best and if possible to give their reason.

An obvious difficulty with this method is that the children's choice and to some extent the reasons they give will be influenced very considerably by the teacher's method of studying the poem. But it is not difficult to detect those answers which are sincere and genuine and those which are mere repetitions of the teacher's statements, and the very different answers given by children in the same class show that, in the majority of instances, the children are expressing their own spontaneous attitude to the poem and giving their own genuine reasons for their choice. Undue influence by the teacher is easily seen by the similarity of the children's answers in any one class.

Another difficulty is that while a child may be able to say what she likes she may not be able to say why she likes it. This is undoubtedly a difficulty with the youngest children but no pressure was brought to bear on them and some gave no reason at all. Where a reason was given, although possibly it does not represent the entire appeal which the poem makes to the child, still it shows at least one reason which the child considers to affect her liking of the poem. The older children expressed themselves freely and it is not difficult to discover their attitude to the poems. The majority of the answers bear the marks of spontaneity and sincerity.

II. CHILDREN'S PREFERENCES.

The method adopted makes it impossible to draw any definite conclusions as to which poems, or which types of poems, appeal most to children of these ages. But one or two points of some interest to teachers may be noticed.

Teachers frequently assume that the type of poem which children like best, especially children in the lowest standards, is the fairy poem. As regards Standards I and II (three classes) fairy poems are certainly found to be very popular, and the reasons given show the children's interest in fairies. Many say, for instance, that they like a poem "because it is about fairies and I like to hear about fairies," "because there is a fairy in it." But one ventures to wonder whether the popularity of fairy poems has not been exaggerated. Fairy poems are bound to figure prominently in the list of preferences because of the large number of such poems included in the lists submitted to the children. But an examination of the children's reasons shows that the fairy element is not the only element in the poem that makes appeal, or necessarily the deciding factor in the choice. A fairy poem may be liked "because it has a buttercup in," "because it is funny," "because the little fairy kissed the little brown rabbit on his nose."

Again, where poems of different types are presented to the children the fairy poems are not chosen by nearly such a large majority as one would expect.

The results, for instance, of one Standard III are as follows:

The Children's Picnic, 19; The Pedlar's Caravan, 12; Dandelions, 9; A Fairy went a Marketing, 4; The Butterfly's Ball, 3; Wynken, Blynken and Nod, 1.

One Standard IV gives the following results:

The Child Next Door, 21; Mrs Brown, 18; The Witch, 5; Abou Ben Adhem, 4; Fairies, 3; The Elfin Artist, 1.

Both these sets of results illustrate the popularity of the poem about ordinary everyday experiences of the child's life as contrasted with the fairy poem, and the same point is brought out clearly in the reasons given for the choice of these poems:

Because it is something that happens every day with children.

Because it is about a little girl who is very proud.

Because it is about things we do.

Because it is about a little girl pretending to be Mrs Brown... because she pretends to have a family of six and still she thinks they can be allowed to make a great deal of noise like other girls and boys.

Because it is like we sometimes pretend to play at and we give ourselves the names of any lady we know.

Because it is like myself playing at house.

The children's preferences are of course, greatly influenced not only by the type of poem usually presented to them but also by the teacher's own preferences and her methods of treating the poem. This probably accounts for some other surprising results. One Standard III votes overwhelmingly in favour of de la Mare's *Someone*. This poem scores 21 votes, while the next highest score is 7 for *The Song of the Shell*. *The Pied Piper*, which one might expect to be very popular, gets only 3 votes. The reasons given by these children show, moreover, that their appreciation of *Someone* is genuine. Another somewhat surprising thing is the popularity of poems about war and fighting and adventure—the type of poem more frequently given to boys. *Horatius* and *The Burial March of Dundee* score very high marks, while many of the girls show a keen appreciation of the murder scene in *Julius Caesar*. This is shown too in the reasons they give, *e.g.* "It is very exciting because of war and people wiping their swords," "It sends you beautiful pictures of great fights." At the same time descriptive poems are much appreciated, such poems as for instance Noyes' *Snow* which in one Standard V scores more than half the votes, although the experiment was carried out in July. Again Masefield's *Sea-Fever* is the favourite of another Standard V, a class in a school in one of the poorest parts of Leeds.

III. CLASSIFICATION OF THE REASONS GIVEN BY THE CHILDREN FOR THEIR PREFERENCES.

The reasons given by the children for their choice vary very widely. They appear, however, to fall naturally into groups which bear an interesting resemblance to the perceptive types first distinguished by Mr Bullough[1] in the aesthetic judgments on colours, and found also by Dr Myers[2] in the appreciation of simple tones and of complex musical forms, and by the writer[3] in the appreciation of rectangles and of arrangements of geometrical figures.

As one would expect with the more complex material, it is not possible to include all the judgments under one or other of Mr Bullough's four types (*cf.* Dr Myers on complex musical forms and the writer on arrangements of geometrical figures). The following groups may be found:

(1) Judgments referring to the subject-matter of the poem.

(2) Judgments referring to the language and form of the poem.

(3) Subjective judgments—those referring to the effect of the poem on the feelings, impulses, or thoughts of the listener.

(4) Character judgments which describe the poem in terms usually applied to people.

(5) Judgments which refer to the author of the poem.

(6) Judgments based on the imagery aroused by the poem.

(7) Associative judgments.

(8) Objective judgments—those based on preconceived standards.

(9) Pragmatic judgments—those referring to the use to which the poem can be put.

These groups are not entirely distinct; for example, the subjective, character and imagery judgments are all closely related, a language judgment may be of the subjective character or objective type and so on.

IV. DISCUSSION OF THE DIFFERENT TYPES OF JUDGMENTS.

(1) *Judgments referring to subject-matter.*

Many of the reasons given by the children take the form "I like this poem because it is about..." followed by an outline of the subject-matter of the poem. These are most common with the youngest children.

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[1] This *Journal*, vol. II, p 406.
[2] *Ibid.*, vol. VII, p. 68 and vol. XIII, p. 52.
[3] *Ibid.*, vol. XII, p. 253.

Some children do not refer to the main theme of the poem but merely to some isolated detail, *e.g.*:

> Because he clashes the basins like a bell (Rand, *Pedlar's Caravan*).
> Because a baby brown lives in the caravan.
> Because they sailed in a river of misty light.
> Because it is about Captain Cook and he has an iron arm [a very incidental reference in the poem].

Others, a little more advanced, enumerate the things mentioned in the poem:

> Because there is a fairy and two children in it.
> Because it is all about gipsies and caravans and little girls and boys;

while others refer to the theme of the poem:

> Because it is about flowers when they are asleep.
> Because it is about the bold outlaw Robin Hood [this scarcely does justice to Noyes' *Sherwood*, but the poem was clearly much too difficult for Standard III children].
> Because it is about fighting.

Judgments of this last type are found in the highest standards also. Older children often refer, not to the actual incidents described in the poem, but to more general information to be obtained from the poem, *e.g.*:

> Because it shows how they buried people in the olden days.
> Because it gives people an idea of what Rome was like in the olden days.

These judgments suggest a purely intellectual attitude to the poem. The following seem to show more real appreciation of the beauty of the subject-matter:

> Because it shows how bold the Romans of old were and how they were willing to sacrifice their lives for their country if necessary.
> Because of the bravery of Horatius.
> I like the spirit of Balder when the ship was burning.

Judgments of this kind tend, with the more prosaic children to appear in the form of statements of the moral of the poem, *e.g.*:

> Because it shows that people who are ambitious come to nothing in the end.
> Because it shows that everyone could be good and kind if they tried.
> Because it teaches us a lesson, and it teaches us it doesn't matter how many nice clothes you have so long as you are happy.

These judgments, like the information judgments quoted above, do not appear to indicate any aesthetic appreciation of the poem.

A number of the children give as their reason for liking a poem that it "has a good meaning" or they refer vaguely to "the thought behind." As this type of judgment is confined almost entirely to one class and is applied by the children indiscriminately to such very different poems as *The Forsaken Merman*, *Julius Caesar*, *The Lady of Shallot*, *Daffodils* and *Wander Thirst*, one can only conclude that it is a meaningless statement, showing mere parrot-like repetition of the teacher's words rather than genuine and personal appreciation of the poem. It appears to be the result of forcing premature generalizations upon the class and to illustrate the over-anxiety of some teachers to reduce appreciation to words. This leads, not to appreciation, but to the memorizing of verbal formulae of appreciation.

Judgments belonging to this group vary very considerably in the degree of aesthetic appreciation of the subject-matter which they show. Many of the children give an outline of the subject-matter of the poem in a detached way which seems to suggest that they would have liked a prose version of the poem equally well. For them the poem appears to be simply what it is about. For example, the following judgment was given by a Standard VI girl:

> Because it tells how determined the Scots were to have the secret of heather ale, the poets also were determined not to tell, the last two poets were forced to tell but the father had the son thrown into the sea so he should not tell.

All this seems to show is that the girl was interested in the story. Others however, give an account of the subject-matter in such a way as to suggest real aesthetic appreciation, as, for example, the following judgment by a Standard III child on de la Mare's *Someone*:

> Because it tells of some one who came knocking at my door, and it makes you frightened, and it tells you of the owl crying and the busy beetle tap-tapping on the wall.

This child appears not merely to have remembered the subject-matter but to have entered into the spirit of the poem. A similar judgment is given by a Standard IV girl:

> Because it tells me about the sea and the crazy casement, also it tells me of the wind and the little face at the window and the waving shadows.

On the surface this is merely a catalogue of the contents of the poem, but the selection of detail seems to show that the child has felt the atmosphere of the poem. Whether a matter-of-fact or an appreciative attitude is adopted towards the subject-matter of the poem is often clearly due to the teacher's influence; in some standards one attitude is prominent, in other classes the other is found. That the teacher's influence is not the only determining factor, but that one or other attitude may be adopted spontaneously by the child, is seen from a comparison of the judgments made by different children in the same class, *e.g.* Wm. Canton's *Carol* may be liked by Standard IV children,

> Because it is about Jesus Christ when he was born in a stable, and because he is the Son of God.
>
> Because it tells about Jesus Christ when he was born in a stable, and when he grew up he went about preaching in the world.
>
> Because it is about God's own garden, and because it tells about the starlight stable where Christ the Babe was born.
>
> Because it tells about the snow on the mountains and from God's own garden dropped a rose of gold.

For the child's attitude to the poem to be one of real aesthetic appreciation there must be, not merely an intellectual grasp of the ideas of the poem, but some emotional response, some sympathy with the mood or atmosphere of the poem.

(2) *Judgments referring to the language and form of the poem.*

Even the quite little children show in their papers that they appreciate the sound of the poem as well as its substance. They say, for example, that they like the poem

Because it is like a song and has a nice sound.
Because it is the sweetest poem I have ever heard.
It is sweet and soft and has a beautiful tone.
Because it has a nice swing with it.
It sounds nice the way we say it.

As the children get older, references to the language and rhythm of the poem become more frequent but in some classes there is a wearisome monotony of phraseology which points to mechanical repetition of some formula suggested by the teacher rather than to genuine appreciation; the children are saying what they think they are expected to say, *e.g.*:

It has much expression in it, it has lovely words.
It has good words and a lot of expression.
Because of its expression.
It has many good words and short sentences.
Because of the expression and the way it is made to rhyme.

Again and again the statement recurs "because it has nice words" or "good expression." These judgments are of the purely objective type (see later) and for these children, at any rate, cannot be considered to show any real poetic appreciation; they are mere echoes of judgments delivered to them ready-made by the teacher. This becomes ludicrous when a girl likes *The Forsaken Merman* because "it is nicely worded, nicely sentenced" or another girl gives as her reason for liking *Julius Caesar* "good words, rhymes nice" or "the words are well said, it is a good sense." The girl who likes the play because "the part about the murder is exciting" is probably far more sincere.

Other children, however, show genuine appreciation of the language of the poem, *e.g.*:

The rhythm is very uncommon and gives a fighting feeling.
There are some beautiful words that make you want to feel jolly and alive.
It rhymed so well that it sounded that soldiers were really marching along.
Some of the words make you feel as if your were in the country watching the snow-flakes.
The words "you shall die at dawn" are very dramatic.

These judgments are not of the objective but of the subjective or closely related types (see later). They show at least some appreciation of the spirit of the poem. One notices, too, that in these judgments the language and rhythm are not completely isolated from the substance of the poem; it is not form alone, but form in relation to substance that is appreciated. In the subject-matter judgments which seem to indicate genuine poetic appreciation (see before) one notices also the tendency to quote the actual words of the poem, ("the busy beetle tap-tapping on the wall") which suggests that it is not the bare substance but the whole poem, in both aspects of form and substance that is appreciated. In both subject-matter judgments and language judgments, then, where we find true poetic appreciation, we also find (1) some response to the emotional atmosphere of the poem; (2) a reference not to subject-matter alone, nor to language alone but to "a unity of which substance and form are both aspects[1]."

(3) *Subjective judgments.*

The children's choice of poem is frequently determined by the effect which the poem has on their feelings, or by the impulses or thoughts it arouses in them. Such judgments represent Mr Bullough's subjective or physiological type. (Dr Myers calls it the intra-subjective type.) The group may be further subdivided:

(*a*) *Affective.* The poem is liked because of some feeling or emotion it arouses in the child; *e.g.* "because it is thrilling" or "exciting," "because it makes me feel frightened," or "makes me feel happy," or "scared" or "worried." These judgments become a little less personal when the first person is replaced by the second or third, *e.g.*:

It makes you feel very sad.
Because it cheers people up.
If any one is sad and in low spirits the *Three Jolly Farmers* will cheer them up.

(*b*) *Impulsive.* The poem arouses some impulse or desire in the child. It is liked:

Because it makes you laugh and giggle to yourself.
It makes you do the actions.
It makes me want to dance and be merry.
It makes you feel as if you want to go in a boat.
It makes me want to be dancing with them.
I'd liked to roam with the pedlars and write a book when I come home.

(*c*) *Physiological.* "It sends a thrill through you," "It makes me feel the cold go through me." These are closely connected with judgments of type (6)—those based on the imagery aroused by the poem.

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[1] Bradley, *Oxford Lectures on Poetry*, I.

(*d*) *Intellectual.* A few children refer to the thoughts aroused by the poem:

It makes you think about it.
If you are going on a long sea voyage it makes you think you will be safe.
It's really very interesting thinking which one will win.

Dr Myers in his paper on *Individual Differences in Listening to Music* says, "The lowest kind of beauty is experienced when the subject adopts the purely intra-subjective attitude surrendering himself to the sensory emotional and impulsive effects of the music[1]." So long as the listener's attitude to the poem is purely personal and lacks detachment or "psychical distance" it remains on the level of that of the typical devotee of the melodrama who sheds real tears over the sufferings of the heroine. On the other hand, the teacher will be inclined to welcome this type of judgment. The child who is thrilled as she reads the murder scene from *Julius Caesar,* is aroused to a desire, at least momentary, to go down to the sea by Masefield's *Sea Fever,* or whose feet are set jigging to the rhythm of de la Mare's *Off the Ground* is at any rate on the way to true poetic appreciation.

(4) *Character judgments.*

There are many judgments which fall into the class called by Mr Bullough the "character type" judgment, that is, they apply to the object adjectives which are usually applied to human beings, *e.g.* the poem is characterized as "happy," "jolly," "mysterious," "fanciful," "weird," "it has a frightened sound," "it has a good strength," as "cheerful," "sad," "sorrowful," "bright," "solemn" or "gay," as "jolly, daring, bold," "cunning," "lively," "silly," "fully of laughter," "gentle," "comical," "dainty." In most instances the adjectives are applied to the poem as a whole; in some they are applied to the subject-matter, *e.g.* "the story is sad," in others to the language and rhythm, *e.g.*:

The words seem jolly.
The rhythm makes it sound more cheerful.
The sadness and disappointment of the words "Come dear children come away."

These judgments represent a higher type of aesthetic appreciation than the subjective judgment as they are less personal, more "distanced"; the child is not absorbed in her own personal feelings but adopts a more detached attitude to the poem.

Dr Myers finds it difficult to agree with Mr Bullough that the character aspect is derived from the subjective, but in these judgments on poems there certainly appears to be a close connection between the two types and one finds some judgments which it is difficult to class as either subjective or character judgments but which seem to represent intermediate stages between the two. For example the judgment "It makes me feel quite sorry for the poor mer-children" is not purely of the subjective type; it is not merely "It makes me feel sad." Again, some judgments refer not to the feeling aroused in the listener but to the feelings aroused in the characters in the story, *e.g.* "There is excitement among the characters, the merman is sad at the bottom of the sea," "I like it because the mother and daughter are worried about him." One can almost make a series of intermediate stages between the two types, the judgments in the series becoming less and less personal, more and more detached:

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[1] This *Journal,* vol. XIII, p. 67.

(1) "It makes me feel frightened."
(2) "It makes me feel sorry for the poor mer-children."
(3) "Because the little girl is frightened of the witch."
"Because the merman is sad at the bottom of the sea."
(4) "Because it has a frightened sound," is "sad," "jolly."

Another closely connected type of judgment is the one which refers to the mood of the author of the poem (see later).

Dr Myers[1] points out that where the music suggests a story to the listener, for aesthetic enjoyment to be perfect, there must be harmony between three factors, the feelings of the listener, those inherent in the character of the music and those of the persons in the story imagined. The same three factors are clearly involved in the appreciation of a poem, though in the poem the third factor is more essential, less dependent on the individual than it is in music. In the examples already given there is evidence of harmony of two of these factors, *e.g.* "It makes you feel sorry for the poor mer-children," "beautiful mournful words, the merman is sad at the bottom of the sea," "It makes you feel as if you want to dance also. The rhyme also makes it feel jolly and it seems to be springy." In this last example, passed on de la Mare's *Off the Ground*, all three factors are included.

(5) *Judgments referring to the author of the poem.*

Many of the children, in giving their reasons for their choice of poem make some reference to the author of the poem. A few, mainly from one class which is characterized throughout by an imitative type of judgment rather than by sincere appreciation, refer to the reputation of the poet. They include among their reasons that the poem was "written by a famous poet," "written by the world's greatest poet," "written by Gould a very famous poet," "the writer also is one of our best." Such judgments are purely conventional and imitative; the child cannot be in the position to make these statements from personal experience, and to encourage them is merely to help to form in the child the habit, not unknown among adults, of judging the value of a work by the name of its author.

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[1] *Op. cit.*

Other children refer to the workmanship of the poem, *e.g.*:

It is beautifully written; the words are strung together in a wonderful rhythm.
He tries to impress the second verse upon us by repeating it at the end.
It shows that the one who wrote it must have been a very good poet and book writer.
Because the poet compares things.
It is arranged in such a clever way that while it is being read the imaginary waves can be heard.
The description proves him to be a very clever writer.
Because the author has described the burning of the ship so well.

These judgments do not represent a true aesthetic attitude as they are concerned not with the beauty of the poem but with the skill of the poet. Some examples are glaringly unaesthetic, *e.g.* "It is a good way to make a poem up about the sea because it is so hard." Many have a note of insincerity about them, *e.g.* "Shakespeare puts his words so nice and short," "a beautiful play composed of beautiful language and noble thoughts which make you appreciate the poets of England." Criticism of workmanship may come with more developed judgment and serve to heighten appreciation, but premature criticism is destructive of true appreciation.

Several of the judgments refer not to the reputation of the poet nor to his workmanship, but to the light thrown on his interests or character, *e.g.*:

A. Noyes wrote very beautiful poems and it seems as if he was a very cheerful man.
Written by R. L. Stevenson and he wrote it because he liked out-door life so much.
When the man wrote this he must have been in a very jolly mood.
Because it shows that the man who wrote it loved nature.

Some of these are closely allied to the subject-matter judgments, *e.g.* the judgment "Because the author would like to be a vagabond, he liked to roam down country roads and to watch the birds and trees" really means that the poem is liked because it is about open-air life. Those which refer to the mood or character of the poet are closely allied to the subjective and character-type judgments. As suggested above, they seem to represent an intermediate stage between the two.

(6) *Judgments referring to the imagery aroused by the poem.*

Much has been written about the importance of imagery in the appreciation of poetry, and the method of image formation is so frequent in schools to-day that one is not surprised to find that imagery determines many of the children's preferences. As one would expect visual imagery is the most frequent, *e.g.*:

> Because I can picture the sparkling water and the coral shells.
> Because I seem to picture the fairies laughing and giggling over their jokes.
> Because it gives a picture of soldiers going up to the battlefield.
> Because it makes me feel as though I was in a garden and looking at the rose petals all over the grass; I can see the picture of the old gardener raking the petals under the mould.

The children differ considerably in the definiteness of their imagery (as far as one can tell from their written answers) but there is evidence of their appreciation of colour, *e.g.*:

> Because there are yellow flowers in it.
> Because women with bright red petticoats come running out.
> Because the crocus has a pretty colour to look at.

Auditory imagery is also found, though not nearly so frequently, *e.g.*:

> You can hear the sweet murmur it makes.
> It makes you think that you can imagine that the drums are going.
> It rhymes so well, it makes it sound like soldiers marching along.

Kinaesthetic imagery is found in:

> It makes you feel as if you were walking through a wood.
> It makes you feel as if you are fighting.

Images of touch in:

> I can feel the wind when it comes in.

And images of smell:

> I seem to smell the sea and feel the breeze as it comes in.

In almost every case the imagery is strictly relevant to the subject-matter of the poem. One girl, however, gives as her reason for liking Masefield's *Sea Fever*, "I seem to smell the sea and feel the breeze as it comes over the sea on to my face. I seem to imagine a little boat with white sails sailing on a blue sea towards a land of adventure where there are cannibals and treasure in some corner of the island." This girl is obviously of an imaginative type; there is evidence of imagery of smell and touch and of visual imagery of a definite and detailed type. But the imagery bears little relation to the poem which suggests it and she has clearly missed the atmosphere and mood of the poem.

These judgments are, of course, closely connected with the subject-matter judgments; they refer to the substance of the poem. On the other hand, many of them show also an appreciation, possibly not always conscious, of the harmony of language or rhythm and content, *e.g.* "You can hear the sweet murmur it makes," "It makes you feel as if you were fighting."

(7) *Associative Judgments.*

In many cases the choice of poem is determined not by subject-matter or language, nor by the relevant imagery suggested, but by more or less irrelevant associations. Many children refer to personal experiences recalled by the poem; experiences of picnics, of gathering flowers, of Christmas, or games, or of stories they have read, *e.g.*:

> Because it is about a caravan and I like to hear where they roam and what they do, and when I read a book about a caravan I am very interested in it, and when my mother wants me to go to bed I ask her if I can stay up a little longer.
>
> Because it is all about gipsies and they go riding all over the town and sometimes they stop and have a feast and there are motor cars and cake walks too and aeroplanes.

This child is not repeating the subject-matter of the poem but is recalling personal experiences of fairs and gipsies. Other examples are

> Because it is a flower and they grow in the field and you can go and pick them.
>
> Because it is like we sometimes pretend to play and we give ourselves names of any lady we know.
>
> Because it is like myself playing at house.
>
> Because it is all about Christmas and we all get our presents and are all happy.
>
> It reminds me of winter.
>
> Because it reminds me of the market.

This type of judgment is related to the preceding type, those based on imagery, and also seems to bear some relation to the intellectual subjective type (those based on the thoughts called up in one's mind by the poem) but it seems to be of distinctly less value from the aesthetic point of view as the associations aroused are to a greater or less degree extraneous to the poem. This is seen very clearly when Noyes' *Snow* is liked because "snow is nice to play in." Thus, while associations with the children's experiences may reasonably help to determine the teacher's choice of poems for her class, they cannot be considered as leading to real poetic appreciation.

(8) *Objective Judgments.*

Many of the children's judgments are of the objective type; the poem is judged by its conformity to some preconceived standard. Many of these judgments are extremely crude; the poem is liked because it is "short" or "long," "neither too long nor too short," "it is a nice length," "the lines are not too long," etc. These, as one would expect, are much commoner with the little children but they are not entirely absent from the higher standards.

Others like the poem "because it is easy to learn," "because I can say it better than the others," "because it is easy to understand," "because it is easy to learn and there are no hard words."

Others use general words of commendation such as pretty, beautiful, fine. Many of the judgments referring to the language and rhythm are of this objective type, *e.g.*:

> The words are suitable and the language is not slang.
> It is excellent for its rhyming at the end of each line.
> Because it rhymes nicely and goes with a swing.
> Because it has a nice smooth rhythm.

The objective attitude, implying, as it does, reference to preconceived standards by which every poem is judged, is essentially a critical, rather than an appreciative attitude. As elementary school children are scarcely in a position to have formed critical standards for themselves, judgments of this type cannot be considered of any aesthetic value. Criticism must follow, not precede, appreciation.

(9) *Pragmatic Judgments.*

Some children refer to the use to which the poem can be put; *i.e.* their judgments are of the type called by Dr Myers the pragmatic type. Under this head would come such judgments as

> I like it because you can sing at the end of each verse,
> Because of the things you can do with it,

and the frequent references to the acting of the poems, *e.g.*:

> Because we act it, we get dressed up and have a wand and a crown on our heads.

Sometimes it is clear that the chief interest of the poem lies in the acting, *e.g.*:

> We act it, and there is a sunbeam in it, and they call the sunbeam Nellie A, they act it very nicely.
>
> Because we act it and there is a lot of little girls watching it and if it is nice we give them a clap.

Similar judgments are also found occasionally in the upper standards, *e.g.*:

> Because it looks effective when we act in it; I like the way they turn round and round and lie on the floor; I like the old man's part; I like the wind because it is so gracefully done.

Here it is clear that what is appreciated is not the intrinsic qualities of the poem but the gracefulness of the movements of the actors. One ventures to wonder how far, except where the poem is dramatic, such acting really furthers the appreciation of the poem, and the method becomes an absurdity when a class of Standard VI girls is set to act a lyrical poem like Masefield's *Sea Fever*.

V. INDIVIDUAL DIFFERENCES.

It is clear that the judgment a child passes on a poem will be the outcome of two sets of factors, the child's spontaneous attitude to the poem and the teacher's influence. There is abundant evidence of the important part played by the teacher in the uniformity of the type of judgment given by some classes, notably by one class, mentioned previously, the children of which give almost without exception judgments of the conventional and objective type. In other classes the teacher's influence is not so marked and one finds more variety in the judgments given. Examples of the different attitude which may be adopted by children in the same class to the subject-matter of the poem have been given above. It is often found that different children in the same class will give an entirely different type of judgment, *e.g.*:

> It gives a very good description of a youth's longing to go down to the sea; the rhythm of the poem goes almost with the movement of the sea; it is excellent for its rhyming at the end of each line; it gives a good idea to those who have never seen the sea; it is a very good way to make up a poem about the sea as it is so hard.

This child clearly has little real appreciation and her attitude throughout is objective. We may contrast with this,

> Because I myself love the sea, because it describes the intense devotion of the king so realistically; because of the gallant speeches; because I appreciate tales with fire and passion in them; because the king is so courageous.

This child adopts an emotional attitude and her judgments are all of the subjective, or of the character type.

Another in the same class of a similar type is the following:

> It begins very mysteriously; the rhythm is very uncommon and gives a fighting feeling, it finishes very surprisingly, all the characters are very interesting; everything seemed peaceful at the end.

In another class we find the following judgments passed on Noyes' *Snow*:

> (*a*) Beautiful words, nice swing, snow is nice to play in, snow is a nice title because it makes you think of winter, the words are nicely put together, repeated words, it has a nice rhythm.

Here the judgments are either associative or objective.

> (*b*) It makes me feel the cold go through me; it makes me feel I am walking through a wood and the sun is dazzling my eyes.

Here the judgments are of a physiological subjective type.

> (*c*) It makes you feel cold and as if the sky were very dark and dull; the beautiful words in it that the snow was coming down from heaven very slow; it makes you think of the great bare trees in the woods with snow round them.

Here we find the subjective type combined with reference to the imagery of the poem.

In the one or two classes where an unnatural similarity in the children's responses points to undue influence on the part of the teacher, it is interesting to notice that the judgments are almost entirely of the objective type and usually an objective language judgment is given such as "it has nice words," "beautiful expression," "the words are nice and short." In many cases the judgments are based on the reputation of the poet. They seem to confirm the view that for these children the objective judgment is not a genuinely aesthetic one; in the majority of instances it is purely imitative.

It is interesting also to notice how frequently the objective and the associative judgments and the bald judgments referring to subject-matter are found together, while the subjective, character and imagery judgments are frequently combined. Also the judgment referring to language and rhythm alone is usually of the objective type, *e.g.* "It is excellent for its rhyming at the end of each line," while where subject-matter and form are combined the judgment is more frequently of the subjective or character type, "It rhymed so well it made you feel that soldiers were really marching along."

VI. SOME EDUCATIONAL CONCLUSIONS.

As the teacher plays a large part in determining which attitude a child shall adopt towards the poem that is being studied, it is imperative that she should consider carefully what is the most desirable attitude for the child, what attitude approaches most nearly to the truly aesthetic attitude, what attitude shows real poetic appreciation.

The objective attitude, which judges a poem by its conformity to certain accepted standards, and which is described by Dr Myers as "the refuge of the untrained...and the resource of the artist,"[1] is, as far as children are concerned, without aesthetic value, for the standards by which the poem is judged are conventional not personal standards. A conventional and imitative attitude is also found in many of the references to the author, especially in the references to the poet's reputation, and often in the criticisms of the workmanship of the poem, as well as in many of the objective judgments passed on language and rhythm. These illustrate the danger of encouraging in the child a prematurely critical attitude. Critical analysis before the child has had abundant genuine aesthetic experience leads to imitation, to insincerity and at times to a most undesirable priggishness. The aim of the teacher should be, not to give the child knowledge about the poem but to help her to gain aesthetic experience of it. The essential thing is that children should really appreciate, not that they should be able to say why they appreciate. Later, criticism may enrich appreciation, but premature criticism leads to a purely intellectual, rather than an emotional attitude. There seems to be considerable danger of the teacher attaching

[1] *Op. cit.*

too much importance to the verbal expression of appreciation and thus leading the children to accept, at second hand, ready-made generalizations instead of genuinely appreciating the poem for themselves.

The most important types of judgments from the teacher's point of view appear to be the imagery judgment and the emotional judgments, the subjective, the character, and those judgments which refer to the mood or character of the writer. The subjective judgment, which, because of its lack of distance is usually considered of little aesthetic value, seems to be the first type of judgment which can with children be said to represent an aesthetic attitude; it is the first evidence of emotional, rather than intellectual, response to the poem, though it lacks the detachment which is found in the character judgment and to some extent in the judgment based on the mood of the poet.

An important point for the teacher to consider is whether the children's attention should be directed principally to subject-matter or to language and form. The literature teacher to-day has come to recognize that the poem is not merely a collection of intellectual facts in verse form, that the aim of literature teaching is not accomplished when the child knows what the poem is about, but that language and rhythm are essential features. More and more attention is paid to the language and form. Mr Greening Lamborn, for instance, says in his *Rudiments of Criticism* (p. 28) that children should be made to understand that "art criticism is concerned not with the matter but with the manner of expression." While the crude statement of the substance of the poem, what information it gives or what lesson it teaches, certainly does not represent an aesthetic attitude, neither, on the other hand, does the equally crude statement of certain facts about language and rhythm necessarily show aesthetic appreciation. Many such statements are of a purely objective type. As Bradley[1] points out, it is only by a process of reflective analysis that form and content can be thought of in isolation, and wherever this analysis is forced prematurely on the children, aesthetic appreciation is destroyed. Teachers might do well to give less attention to isolated discussion of language and form, to encouraging the children in the use of such formulae as "it has nice words," or even to selecting the nice words, and more attention to stimulating the right kind of appreciation of subject-matter, to arousing genuine enjoyment of beautiful and poetically expressed ideas.

[1] *Op. cit.*

(*Manuscript received* 5 *February*, 1927.)

A WORKED OUT EXAMPLE OF THE POSSIBLE LINKAGES OF FOUR CORRELATED VARIABLES ON THE SAMPLING THEORY

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1. *Introduction and Aim.*
2. *A Universe of* 108 *Elements.*
3. *A Universe of* 6 *Elements.*
4. *A Universe of* 12 *Elements.*
5. *Conclusions.*

1. Introduction and Aim.

As is well known, the interrelationships of a tetrad of correlated variables are of importance in theories of the structure of the intellect, and the quantity

$$F \equiv r_{13}r_{24} - r_{14}r_{23}$$

has been made a touchstone for deciding the validity of certain hypotheses. The aim of the present article is to examine all the possible values, in their proper frequencies, which this tetrad-difference can take in certain simplified examples, to illustrate the manner in which chance linkage will distribute F.

These examples are all of the following form. Let there be a universe of A elements, all-or-none in nature, and quantitatively equal in their effects, of which universe a_1, a_2, a_3, and a_4 are samples of those numbers respectively of such elements. To clarify ideas (though the illustration will limit and coarsen the possibilities), A may be thought of as the totality of neurones in the cortex, a_1 the neurones concerned in the first activity, and so on.

2. A Universe of 108 Elements.

As a first example, let

$$A = 108; \quad a_1 = 90; \quad a_2 = 72; \quad a_3 = 54; \quad a_4 = 36.$$

Then very many kinds of possible linkages between the four variables are possible, and numerous different tetrads of correlation coefficients. But if a_1 and a_2 are really random samples, that is, independently formed samples, of A, then a_2 will have *most probably*

$$\frac{72}{108} \times 90 = 60$$

elements in common with a_1. Similarly a_3, which has 54/108ths of the whole A, will have *most probably* that same fraction of each of the categories formed by the existing variables a_1 and a_2; and so on. In short, the most probable distribution of linkages is as follows:

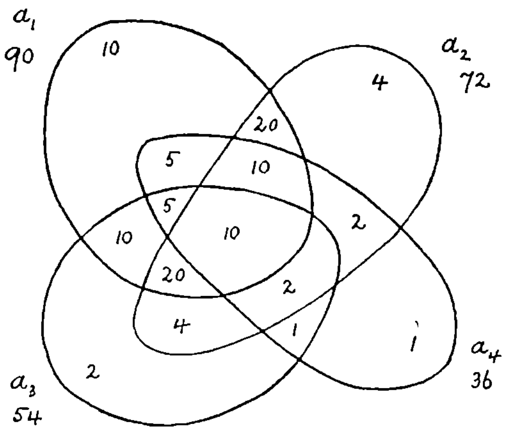

Fig. 1.

(The total number within the diagram is 106, *i.e.* 2 elements, *most probably*, escape being involved in any one of the four variables.)

The six correlation coefficients which this most probable structure would give are

	a_1	a_2	a_3	a_4
a_1	•	·75	·65	·53
a_2	·75	•	·58	·47
a_3	·65	·58	.	·41
a_4	·53	·47	·41	•

wherein each r is given by

$$\frac{\text{common elements}}{\text{geometrical mean of two totals}},$$

i.e.
$$r_{12} = \frac{60}{\sqrt{90 \times 72}} = 0{\cdot}75.$$

The three tetrad-differences of the above square table of correlations are each equal exactly to zero, as may be accurately ascertained by using the vulgar instead of the decimal fractions. The most probable result of forming variables by random sampling of such underlying elements is $F = 0$[1].

This, however, although the most probable result, is not the only possible result. Actual sampling in the above instance (sampling, that is, of the underlying factors) would give values of F distributed about zero, with a mode at zero. Fig. 1 is the most probable pattern[2]: but to give all the other possible patterns, find their relative frequencies, and calculate their tetrad-differences would be extremely laborious for this case of $A = 108$. I have, however, carried out such a calculation for the two simpler cases in which the variables a_1, a_2, a_3, and a_4 bear the same relative proportions but (1) where $A = 6$ and (2) where $A = 12$; and give the results in summarized form below. In both these cases the most probable pattern is impossible, to use an Irishism. I mean that when $A = 6$, and a_1, a_2, a_3, a_4 equal 5, 4, 3, and 2 respectively, a pattern like Fig. 1 but with smaller numbers is impossible, because, for example, a_4, which contains 2/6ths of all the elements, obviously cannot contain 2/6ths of a_1, because a_1 has 5 elements and 5 is not divisible by 6. Yet, although the smallness of A prevents the occurrence of what would otherwise be the most probable distribution, it will be found that the average value of F is still exactly zero: and that the distribution of F about zero decreases as A increases and can therefore be made as small as we like.

3. A UNIVERSE OF 6 ELEMENTS.

Let $A = 6; \quad a_1 = 5; \quad a_2 = 4; \quad a_3 = 3; \quad a_4 = 2.$

There are then 78 possible patterns of linkage between the four variables: and, when each of these is taken in its proper frequency, 450 instances are required to give a frequency of *once* to the least probable patterns. There are two such least probable patterns, each occurring only once in 450 instances, one of them, for example, being shown in Fig. 2.

On the other hand, a more probable pattern, occurring 12 times in 450 instances, is shown in Fig. 3.

Fig. 3 also illustrates a pattern which gives each of the three tetrad-differences exactly equal to zero. Another pattern also does this, with three instances, making 15 instances in all, out of the 450, where all three F's equal zero.

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[1] See this *Journal* (January 1927).

[2] For an explanation of several terms used in this paper, see p. 73.

No pattern occurs more than 12 times. But in certain cases two different patterns give identical correlations, and the greatest frequency of any one sextet of correlation coefficients is 18 times out of the 450.

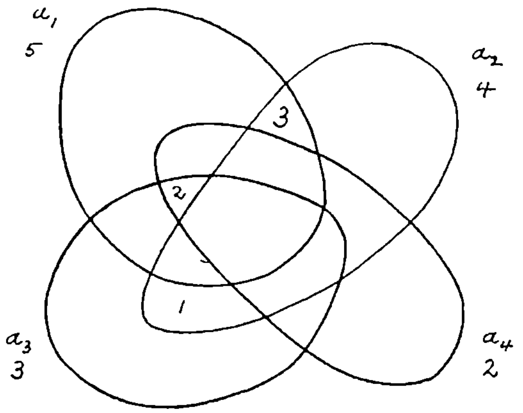

Fig. 2.

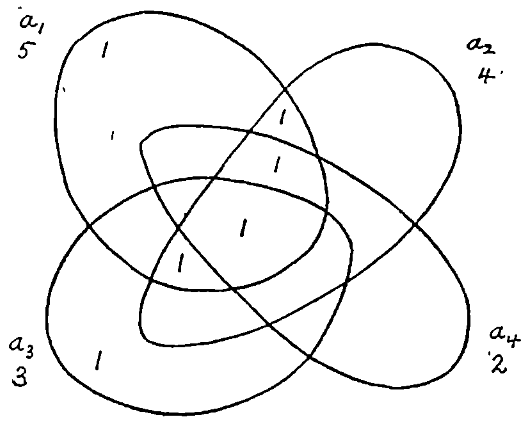

Fig. 3.

There are, as I said above, 78 different patterns: but only 76 different sextets of correlations. These 76 different sextets of r, each sextet or *variety* taken with its proper relative frequency, give the following distributions of F_1, F_2 and F_3 where

$$F_1 = r_{13}r_{24} - r_{23}r_{14};$$
$$F_2 = r_{12}r_{34} - r_{23}r_{14};$$
$$F_3 = r_{12}r_{34} - r_{13}r_{24}.$$

Values of $F \times \sqrt{120}$	Frequency of F_1	F_2	F_3
8	.	.	2
7	.	4	0
6	.	8	14
5	9	2	6
4	27	34	23
3	6	12	30
2	75	72	48
1	61	66	72
0	99	54	81
−1	56	78	36
−2	67	42	42
−3	16	30	60
−4	30	36	18
−5	0	0	0
−6	4	12	18
450		450	450

The distribution of F_1 is shown graphically in Fig. 4.

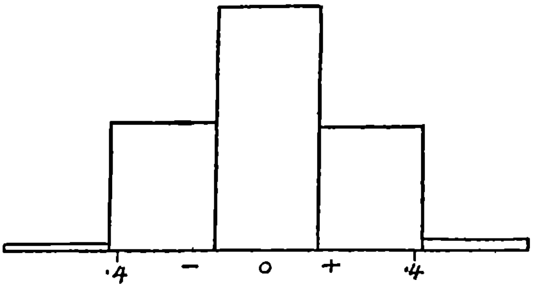

Fig. 4.

Although the distribution of each F about zero is slightly irregular, the average value of each F is *exactly* zero. For F_1, the variance

$$\sigma^2 = \frac{2164}{120 \times 450} = 0{\cdot}040.$$

We shall see that when the numbers of elements are, as in the next section, doubled (the proportions in the four variables remaining the same), this variance is, approximately, halved.

4. A UNIVERSE OF 12 ELEMENTS.

Let $A = 12;\quad a_1 = 10;\quad a_2 = 8;\quad a_3 = 6;\quad a_4 = 4.$

That is, all the numbers in the preceding section are doubled. The complexity of possible linkage patterns is thereby very much increased, F still retains exactly the average value zero, and σ_F is reduced.

There are now 1257 different sextets of correlation coefficients possible, and a still greater number of structure patterns, for, as in the last section, different patterns sometimes give the same correlations, and these families of patterns become more numerous as the universe A is increased.

When each sextet is taken in its proper relative frequency (ranging from once to 11,520 times) there are no less than 1,078,110 instances required to represent the distribution.

For clearness I am employing in this paper the words *sextet* and *variety* to mean a certain set of six correlation coefficients. The word *pattern* refers to the structure diagrams, and several patterns, agreeing in their correlations, may form one *variety*. The word *instance* refers to frequency, either of patterns, or varieties, according to context. The varieties are, in the ledger in which I have recorded the whole of this calculation, entered in *groups*, the varieties in each group being alike in r_{12}, r_{23} and r_{31}. There are 45 conceivable groups, but of these 8 do not occur, so that there are 37 actually existing groups and of these 3 subdivide each into two, according to fundamental differences of pattern. The groups differ in their relative frequency, and in the number of varieties they contain. For example, the record of group 13 is as follows. Let c_{12} = the number of elements common to a_1 and a_2, and so on. Then in group 13, $c_{12} = 6$, $c_{13} = 4$, $c_{23} = 4$. Within the group, c_{14} may have the values 4, 3 or 2, and within each of these sections c_{24} and c_{34} may have values from 0 to 4, with frequencies as follows:

$c_{14} = 4$:

				c_{24}		
		0	1	2	3	4
	0	.	.	6	8	1
	1	.	8	32	32	8
c_{34}	2	1	12	39	32	6
	3	.	4	12	8	.
	4	.	.	1	.	.

$c_{14} = 3$:

				c_{24}		
		0	1	2	3	4
	0	.	.	.	.	.
	1	.	.	8	24	8
c_{34}	2	.	4	36	56	24
	3	.	4	24	36	8
	4	.	.	4	4	.

$c_{14} = 2$:

				c_{24}		
		0	1	2	3	4
	0	.	.	.	.	.
	1	.	.	.	.	.
c_{34}	2	.	.	1	8	6
	3	.	.	4	12	8
	4	.	.	1	4	1

These tables are, it will be seen, partial correlation tables, the four correlations r_{12}, r_{23}, r_{31} and r_{14} being constant. There are in all 38 varieties within the group, that is, 38 cells of the above three tables possess contents. These cell contents are the frequencies of each variety within the group: but as the group itself has a relative frequency of 90, they have to be multiplied by that number to give the total number of instances of each variety.

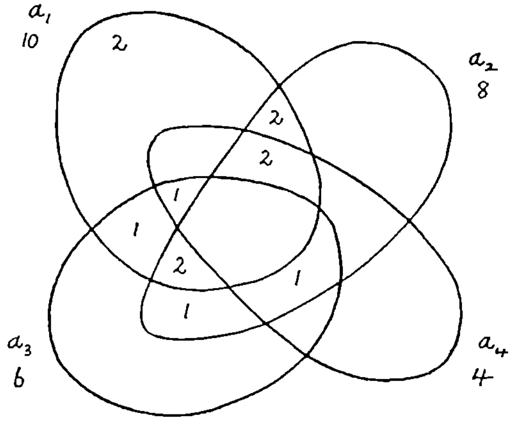

Fig. 5.

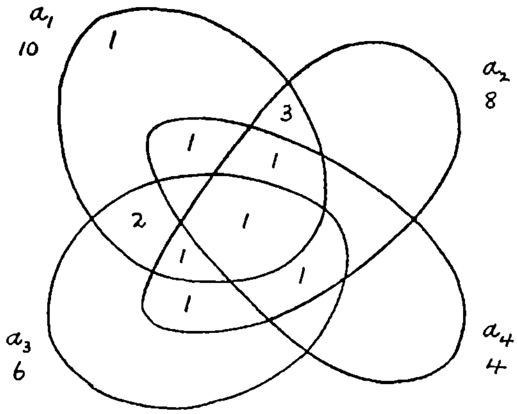

Fig. 6.

One variety of group 13 happens to give each tetrad-difference equal to zero, viz. that which has a frequency of 56 × 90; for this reason the 56 in the above tables is printed in heavy type. It has two patterns, as shown in Figs. 5 and 6, which occur in the relative frequencies 3 : 4.

These remarks and diagrams may serve to indicate the nature of the calculation, which when completed enables all the possible tetrad-differences to be calculated in their proper frequencies, for this universe of 12 elements or "neurones," and for these four variables involving 10, 8, 6, and 4 such "neurones." The distribution of F_1 is as follows:

$\sqrt{1920}\,F_1$	Freq.	$\sqrt{1920}\,F_1$	Freq.	$\sqrt{1920}\,F_1$	Freq.	$\sqrt{1920}\,F_1$	Freq.
20	225	7	17,760	−3	31,432	−13	624
18	1,800	6	74,392	−4	72,676	−14	3,792
16	1,755	5	15,744	−5	53,808	−15	4,144
15	4,600	4	52,085	−6	49,328	−16	3,970
14	3,840	3	121,608	−7	21,240	−18	112
12	19,610	2	42,384	−8	41,951	−19	456
11	10,632	1	28,096	−9	5,896	−20	584
10	8,360	0	122,699	−10	29,184	−24	28
9	26,696	−1	63,024	−11	8,960		
8	37,735	−2	81,208	−12	15,672	Total	1,078,110

This table gives an average value of F_1 exactly equal to zero. With a little grouping the distribution gives Fig. 7.

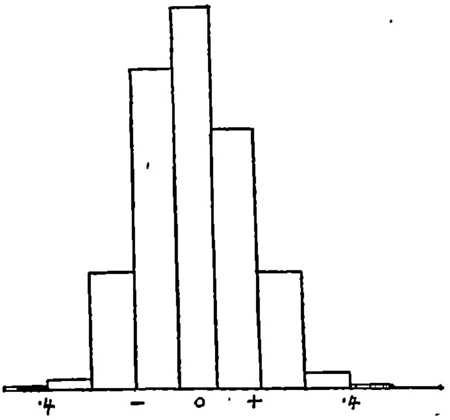

Fig. 7. (Compare Fig. 4.)

The variance of the distribution is given by

$$\sigma^2 = \frac{37,166,400}{1920 \times 1,078,110}$$
$$= 0{\cdot}018.$$

This is less than half the variance 0·040 of the previous example in which $A = 6$ (whereas here $A = 12$, and all numbers of elements are doubled). It is a matter of some surprise to me that this variance should not be exactly one-half the former, but there does not appear to be any error in the calculation. Just as, in binomial distributions, and in doubly binomial correlation tables, certain laws which are exactly true of normal curves and surfaces are only true approximately or on the average (for example, such a correlation table is not homoskedastic, but the weighted variance of the arrays is "correct"), so here I have little doubt that with larger "universes" the variance is exactly in inverse proportion to the number of "neurones" in the whole.

5. CONCLUSIONS.

The whole example strengthens me in my frequently expressed belief and contention that correlation coefficients tend towards zero tetrad-differences without any need of the assumption that only a general factor and specific factors exist, to the exclusion of group factors. This is, at any rate, certainly true where the underlying fund of factors is composed of all-or-none elements quantitatively equivalent in their influence. Variables composed of samples drawn at random from such a fund will have extremely complex linkages, including group factors of all degrees: but their tetrad-differences will average zero, and σ_F will be the smaller, the more elements there are in the "universe" or fund from which to draw. The "brain" of the above examples contains only 6 "neurones" (in the first example) and 12 "neurones" (in the second). With a number of "neurones" at all approaching that in an actual brain σ_F would be very small indeed.

I have spoken of "neurones" mainly for the sake of vividness, but also because they are actually, I understand, all-or-none in their action. If other elements are thought of, whether psychological (as, say, primary instincts) or physiological (as, say, glandular products), they are not so likely perhaps to have this property. I am not convinced, however, that it is necessary, though I cannot give conclusive proof. But it seems to me that where any activity or ability can be considered as being dependent upon a large number of small influences, there will be at least an approach to the conditions assumed in this paper, and therefore to tetrad-differences closely grouped about zero, without any further hypothesis being needed. Where the causal influences are few, F will be more widely scattered; where they are numerous, it will have a narrower spread.

(*Manuscript received* 6 *December*, 1926.)

PROFESSOR McDOUGALL AND THE LAMARCKIAN HYPOTHESIS

By V. HAZLITT.

(*From the Psychological Laboratory, Bedford College, University of London.*)

1. *The Experiment* (pp. 77, 78).
2. *Tank Test Results* (pp. 78, 79).
3. *Bath Test Results* (pp. 79, 80).
4. *Control Experiments* (pp. 80–82).
5. *Comparability of Groups* (pp. 82–85).
6. *Summary* (pp. 85, 86).

1. *The Experiment.*

In this *Journal*, vol. xvii, pp. 267–304, Prof. McDougall gives an account of an experiment which he has been carrying out for the last six years to test the Lamarckian hypothesis of the inheritance of acquired characteristics. In 1921 he obtained six female and three male rats from the Wistar Institute. The stock from these he divided into two halves as nearly alike as possible in vigour and other qualities. These two halves were named T and B respectively, and each of these half-stocks was strictly inbred for the 17 generations with which the experiments are concerned. Prof. McDougall's plan was to train the different generations of the stocks T and B in a specific activity before breeding and to test each successive generation to see if it showed any tendency to perform the task without training, or to learn the performance with less training than its ancestors, and also to compare the performance of each half-stock at its 'hereditary' task with the performance of the other half-stock which had had a different 'hereditary' task or with the performance of other rats. The tasks finally chosen were:

(*a*) to escape from a water puzzle-box called the 'tank' in which choosing a lighted passage was punished with electric shocks and choosing a dark passage led to escape from the water;

(*b*) to escape from a water maze called the 'bath' by diving under partitions at certain points and avoiding blind alleys.

Prof. McDougall holds that the results of both series of experiments give support to the view that acquired characteristics may be transmitted, but he places greater confidence in the Tank than in the Bath results. He says, "I submit that the records of the testing and training of the Tank rats do seem to constitute evidence of such transmission and that, unless this appearance of Lamarckian transmission can be explained away in some fashion which hitherto has not occurred to me, they must be accepted as such evidence."

The experiment afforded two lines of evidence—that based on a comparison of the work of earlier with the work of later generations, and that based on a comparison of the performance of each half-stock with control rats that had had no training, or different training in past generations. With regard to each of these lines of evidence two criteria might be used: the initial performance of the rat before he was trained, or his rate of learning. In relation to the Tank experiment both criteria were used separately; for the Bath experiment they were amalgamated. The comparison of later with earlier generations may be made from the following tables:

Results for Tank Experiment.

If there were transmission of the tendency involved, later generations should choose the dim passage more often than earlier generations.

	Choices	
Generation	Dim	Light
10th	101	73
11th	122	124
12th	55	41
13th	76	62

Results for Bath Experiment.

If there were transmission, the rats of the later generations should take a shorter time than the rats of earlier generations.

Generation	Average time in seconds
1st	2320
2nd	1340
3rd	1540
4th	1950
5th	1180
6th	1610
7th	2240
8th	980
9th	—
10th	1980
11th	1250
12th	1140
13th	—
14th	1480
15th	1020
16th	1020
17th	1620

2. *Tank Test Results.*

Prof. McDougall does not say much about these figures as a series, although when he makes the comparison between these T rats and the controls he treats them as affording evidence that the T rats had inherited an aversion for the bright gangway. If, however, on the basis of the figures given, we work out the percentage of choices of the dim passage for each of the generations, we find the following:

10th	generation made	58	per cent.	choices of the dim gangway.
11th	,,	50	,,	,,
12th	,,	57	,,	,,
13th	,,	55	,,	,,

These percentages do not show any sign of increase in the tendency to be acquired and would only have significance if the ancestors of these rats had at the beginning of the experiment shown a strong preference for the lighted gangway. The figures for the 1st generations are not given, but those for the 7th are. They show that at the 7th generation the T rats chose the dim gangway 84 out of a total of 156 times, *i.e.* 54 per cent. If this 54 per cent. in the 7th generation is compared with 55 per cent. in the 13th, it is seen that seven generations have brought about no real strengthening of the tendency to choose the dim passage. This being the case, it is difficult to believe that the first six generations had changed much in this respect. This brings us to the conclusion that there was a tendency in this half-stock to choose dark a little oftener than light from the first—a possibility that Prof. McDougall recognizes on a later page.

3. *Bath Test Results.*

The table of results for the first twelve days of training in the Bath for the successive generations of B rats gives at a glance an impression of improvement with the later generations, but the results are very irregular; the 8th generation has a better record than any of its successors, up to and including the 17th, and the 2nd generation has an average little worse than that of the 14th, 15th, and 16th together (omitting the 17th because it was exceptionally slow from special causes). Prof. McDougall realizes that the figures for the averages are not conclusive and emphasizes in preference to these the result of averaging in turn the records for the quickest rats in each of three generations. This procedure gives the following results:

2nd	to	4th	generations	1002	sec.
5th	,,	7th	,,	1010	,,
8th	,,	10th	,,	975	,,
11th	,,	13th	,,	846	,,
14th	,,	16th	,,	787	,,

This result seems to support Prof. McDougall's conclusion. But is the improvement due to the inheritance of an acquired characteristic or to the fact that there was selective breeding? The litters numbered about twelve, and six of these were killed within the first week to give those that were bred a better chance. This may be a good plan, but unless it had been followed for some generations before the beginning of the experiment it would lead to a gradual improvement of the stock during the course of the experiment, especially as the runts and obviously delicate rats were killed, although there was no other form of selection. This matter seems worthy of attention in view of the fact that Prof. McDougall says that, "vigour contributes importantly to excellence in the task of the half-stock B." It has been proved that inbreeding alone leads to increased vigour in white rats[1]. In addition to inbreeding there is to be taken into account in the case of these rats the very good conditions at the early period. Prof. McDougall's figures suggest that there were some outstanding individuals in the later generations, but the figures show no such improvement on the average as one would expect if there were "Lamarckian transmission and accumulation of the effects of training in the shape of innately increased facility of learning or acquisition." That there was not such improvement is shown by the fact that the average rat of the 2nd generation performed his task in a time that was 8 per cent. shorter than the average of all succeeding generations, and that the quickest rat of the 2nd generation performed it in a time that was 6 per cent. shorter than the average of the quickest rats of all succeeding generations.

4. *Control Experiments.*

The problem of control experiments in such a study as this is always peculiarly difficult to solve satisfactorily for the following reasons:

(*a*) In regard to statistical requirements. There are such wide individual differences amongst rats (a fact noted by Prof. McDougall, p. 273) that unless large numbers of both the main and the control group are tested, the results are liable to be unrepresentative.

(*b*) In regard to comparability of groups. Degree of tameness and being accustomed to the surroundings have such an important influence upon the performance of the rats that unless the two groups are very much alike in these two respects the results cannot be compared.

In order to consider Prof. McDougall's success in meeting the first of these requirements, we may take his Table VI, on which he says that he "chiefly relies in claiming that his experiment has produced evidence supporting the Lamarckian hypothesis." This table shows that with the 23 T rats of the 13th generation, an improvement of 28 per cent. took place in 30 days, whereas with the 39 control rats the improvement amounted to only 11 per cent. This contrast may support the assumption that there was a difference between the T rats and the controls, but as it stands it would not be very certain evidence from the statistical point of view because the numbers were small and it is not possible from the data given either in this table or in Table VII to calculate the mean variations for the two groups, although from Table VII one would expect them to be large. Prof. McDougall attributes a much larger difference than that between the T rats and the controls to chance circumstances—59 per cent. difference between the 16th and 17th generations of the

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[1] See *Inbreeding and Outbreeding*, East and Jones, p. 108.

TABLE VI.

Day of training	23 Tank-rats, 13th generation		39 Control rats	
	Dim gangway	Bright gangway	Dim gangway	Bright gangway
1st	*76	*62	99	135
2nd	66	72	102	132
3rd	74	64	112	122
4th	68	70	109	125
5th	71	67	119	115
6th	68	70	102	132
7th	69	69	119	115
8th	68	70	119	115
9th	67	71	111	123
10th	67	71	111	123
11th	72	66	108	126
12th	79	59	110	124
13th	80	58	113	121
14th	81	57	117	117
15th	78	60	111	123
16th	75	63	109	125
17th	80	58	108	126
18th	89	49	112	122
19th	88	50	107	127
20th	97	41	118	116
21st	98	40	123	111
22nd	97	41	117	117
23rd	101	37	108	126
24th	107	31	122	112
25th	105	33	118	116
26th	111	27	122	112
27th	111	27	124	110
28th	116	22	117	117
29th	119	19	121	113
30th	115	23	126	108

* These figures for the first day of training are given as they appear on p. 283. In Table VI they have been reversed.

B rats (p. 299). In a later section the further question will be raised as to whether, even if there were a significant difference, it would necessarily be due to transmission of the acquired characteristic in the T rats.

It cannot be said that the statistical results for the control experiment with the Bath procedure are any more satisfactory than those for the Tank. Prof. McDougall is not satisfied with them. He had too few controls, and as the experiments were planned to correspond with only three of the generations of the main experiment the results were liable to be affected by chance circumstances. It is strange that Prof. McDougall emphasizes the effect of periodic variations upon the performance of the rats in the main experiment and yet makes no allowance for it upon the performance of the controls. He may assume that the fluctuations synchronized in the two groups, but his figures show that this was not the case:

Results for rats in main experiment—5th gen. not so quick as 7th; 7th quicker than 17th.
Results for rats in control group—5th quicker than 7th; 7th not so quick as 17th.

5. *Comparability of Groups.*

The control rats may be considered in two groups: (*a*) those that belonged to the half-stock other than the one employed in the main experiment; (*b*) those that were obtained from the Wistar Institute at the time of the control experiment. In the group of control rats compared with the 13th generation of T rats there were 22 B rats and 17 from the Institute. In the group used in connection with the Bath experiment there were 23 T rats and 6 from the Institute. It seems unexceptionable to use rats of the two half-stocks as controls for each other. They were descended from the same original stock and had similar environments. If, as we have suggested, the stocks were being improved by inbreeding, this presumably affected both half-stocks equally, so that, although it might not be legitimate to compare the performance of later generations with that of earlier, it should be permissible to compare the performance of T rats at their 'hereditary' task with that of B rats who had not had that 'hereditary' task, and *vice versa.* But is it legitimate to do so on Prof. McDougall's theory? He holds that T rats are inheriting a tendency that favours performance of the T task and that B rats are inheriting a tendency that favours performance of the B task. If this is so each half-stock may be handicapped as control to the other; the T rats, who according to the theory are inheriting a tendency to react to differences in lighting when put in the water, may on this account take longer to learn to dive at the right points, and the B rats, who are supposed to be inheriting the tendency to dive, may take longer to learn to react to difference in lighting. In the case of motor habits that are not very different in nature from one another it has been shown that the formation of one habit does not interfere with the formation of another, but in a case somewhat similar to the present one, Hunter did find such interference[1]. As Prof. McDougall maintains that there was inheritance, he must admit that his method in this particular might falsify his results and by putting the controls at a disadvantage enhance the apparent gain of the animals in the main experiment. Such a fault in technique would not affect the argument as to whether the difference is due to transmission or not, it would only make the effect of such transmission appear greater than it actually was.

If we do not hold that there was transmission, we must consider why, as shown in Table VI, the controls did not learn as quickly as the T rats. The difference may not be truly significant, but allowing that it is we may ask, "Did Prof. McDougall ensure that the T rats and the controls were on exactly the same footing so that such a difference could only be due to transmission in the T rats?" The W control rats had had a totally different environment from the T rats, and of the B rats Prof. McDougall says, "...the whole of the handling of the half-stock T was confined to my own hands; *whereas I was compelled to leave the handling of the other half-stock B almost entirely to several of my assistants.*" The T rats were accustomed to Prof. McDougall's handling, the control rats were not. To those who have not worked with albino rats it will seem paltry to mention this; but to those who have worked with them it will seem a very important consideration. Prof. McDougall recognizes how important may be the difference made by such circumstances when he attributes the poor performance of the 17th generation of B rats to a change in the experimenter; in this case he is willing to attribute an average increase in time of 59 per cent. to such a change. Rats 'get used' to certain ways of being handled and very small changes throw them into a nervous state that makes them slow to learn. Not only do differences in tameness tell in this way, but the tameness increases from generation to generation in a given environment, probably because each successive mother becomes accustomed to the various stimuli in the environment and so gives fewer fear reactions while she is with her litter. The young of the later generations thus learn fewer fear reactions in their infancy and become indifferent to some of the stimuli that have caused them fear, before they in turn have a litter.

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[1] *Journ. of Animal Behaviour*, vol. VII.

The subject of the increase in tameness with successive generations is a reminder of a preliminary experiment made by Prof. McDougall to which there has been no reference. In the experiment in question the rat was put into a hollow revolving wheel in which there was an electrified section. The rat had to learn to jump the electrified section regularly as the wheel revolved. At first the rat would run on the electrified section and receive a shock, but in time the sight of the section would cause him to jump and avoid the shock. The 1st generation learned the task fairly easily, but each successive generation up to and including the 4th took longer, and the rats in these later generations tried to avoid the electrified section by running up the side walls instead of leaping over the section. Prof. McDougall ascribes this change in behaviour to an inherited fear. He speaks of Lamarckian transmission bringing about this important change in behaviour *in four generations*, whereas only six pages back in the same article, when he is criticizing Pavlov for suggesting that a change in behaviour could be effected in five generations, he writes, "...if Lamarckian transmission occurred in nature with the strength reported by Pavlov, it would be incredible that we should not already have knowledge of many unmistakable instances of it."

Before seeking to account for the rats' change in behaviour in terms other than those of Lamarckian transmission, it seems pertinent to ask why, if these rats had inherited what might be "a general increase in timidity" (p. 277) through the experience of the earlier generations in the wheel, their descendants were used for the Tank experiment and as controls in the Bath experiment? This fact alone would invalidate the results for both experiments.

It seems at least probable that the changing behaviour of the successive generations of rats in the wheel experiment may be accounted for by increasing tameness coming about in the way suggested in an earlier paragraph. The nervous rats of the earlier generations make the minimum adaptation of jumping the 'shocking' section. The less nervous rats of the later generations adopt a less routine solution. They climb to where they will not be faced with such a trying situation. For, after all, it is unlikely that any rat enjoys having to run continuously and to leap time after time to save himself from shocks, he only does it at the urge of a fear that leaves no possibility of varied response. Prof. McDougall says that in the 3rd and 4th generations the more bold and vigorous continued to leap across the section and the more timid ones refused to face it. But as far as one can tell, the rats who faced the section and leaped were called "bold and vigorous" because they acted in this way. It seems much more likely that it is the less nervous rat who looks before he leaps at some stage in the proceeding and chances upon a better solution of his problem. The rat's solution was probably better than he knew for it finally led the Professor to abandon this trying experiment.

Prof. McDougall stresses the fact that it is almost impossible for one worker or one laboratory to carry out an experiment of the kind that he attempted, and he puts in a plea for an organized experiment on a much larger scale. It is certainly a case where by co-operation amongst a number of scientists, including physiologists, zoologists, and psychologists, a much more satisfactory piece of work might be done than will ever be achieved by a greater number of workers experimenting in isolation. In particular, much might be gained by collaboration amongst workers in the different fields with regard to the choice of suitable tasks. Such an experiment might well throw considerable light on the subject if the following precautions were taken:

1. The rats bred in large numbers, admitting of control groups equal in size and number to the groups in the main experiments.
2. The experiments postponed until several generations have been bred in the laboratory in order that the results might be affected as little as possible by increasing tameness or improvement in the stock.
3. The control groups in each case handled by the same experimenter and differing from the group in the main experiment in only the one respect of having ancestors who did not learn the given task.

6. *Summary.*

The argument of this paper may be summarized under the following heads:

1. Prof. McDougall's results are not convincing from the statistical point of view.
2. The conditions of the experiment might lead to later generations showing an improvement on earlier, and a superiority to controls for reasons other than the inheritance of an acquired characteristic, viz.:

(*a*) Improving quality of the stock due to drastic reduction of litters and to inbreeding.

(*b*) Increasing tameness which, it is claimed, need not involve transmission of an acquired characteristic (see p. 7).

(*c*) Incomparability of control rats with rats in the main experiment.

3. If Prof. McDougall is right in claiming that there was transmission, his technique was faulty in not guarding against interference between habits in the controls and in comparing a group that he held might have inherited a general disposition to fear with others that had not inherited such a disposition.

(*Manuscript received* 30 *June*, 1927.)

PUBLICATIONS RECENTLY RECEIVED

Early Mental Traits of Three Hundred Geniuses. By CATHERINE M. COX, assisted by LELA O. GILLAN, RUTH HAINES LIVESAY and LEWIS M. TERMAN. (Vol. II of *Genetic Studies of Genius*, edited by Lewis M. Terman.) Stanford University Press. 1926. Pp. xxiv + 842. (London: Harrap. 21*s.* net.)

This is the second volume of a work which was designed to find out to what extent, and how, genius is evidenced in childhood. The first volume, a notice of which appeared on p. 259 of vol. XVII of this *Journal*, dealt with the characteristics of over 1000 gifted children. It remains for the future to tell what eminence they achieve. This second volume deals with the complementary problem. The biographies and writings pertaining to 301 eminent men and women, who lived between 1450 and 1850 A.D., have been examined to discover how far their future greatness was foreshadowed in their early lives.

The difficulties of making such a study, and of interpreting the results so that they shall have scientific value, are enormous; but the author and her assistants realized them and overcame them, so far as was possible. For example, the assessment of the intellectual abilities of the subjects is one problem. It is not possible to apply intelligence tests, yet it is desirable that the intelligence of each subject should be expressed numerically. To accomplish this the statements concerning the actual doings of each subject were collected, and were then compared with the performances of present day children of similar age, and so evaluated. The result is that Faraday, for example, of whose childhood little is known, is rated comparatively low (IQ = 105) as a youth; but, more being known of his doings as a young man, this IQ at a later age is higher (150). In this way a minimum estimate is obtained of the intellectual ability of each individual studied. Information regarding heredity, environment, interests, and character traits was similarly collected and assessed.

The conclusions drawn are that those who achieve eminence have, as a rule, great strength and force of character, confidence in their abilities, dogged persistence and (except in the case of soldiers) intellectual ability; heredity and environment are both important.

Details of the enquiry have been well set out in two parts. In Part I (220 pages) the methods and results are described; in Part II the evidence, on which the ratings were made, is summarized. This part is a mine of interesting information. In an appendix one study (of the philosopher Schelling) is given in full, while excerpts from the early writings of the "geniuses" are published in a second appendix.

Common Sense and its Cultivation. By DR HANBURY HANKIN, M.A., with a foreword by Dr C. S. MYERS, F.R.S. London: Kegan Paul, Trench, Trübner and Co., Ltd. New York: E. P. Dutton and Co. 1926. Pp. x + 289. 7*s.* 6*d.* net.

Dr Hankin has produced a most readable volume in defence of 'Common Sense,' by which he means the faculty of arriving at decisions intuitively in the absence of conscious reasons for the decision.

He marshals his numerous and apt illustrations well, and emphasizes his argument with delightful stories of calculating ability, musical genius, confidence tricks, lawyers, business men, quakers, globe-trotters, and a host of others.

There is an interesting chapter on Educational Systems, in which our present system is severely handled, because it lays the emphasis on logical reason.

The Borderland of Music and Psychology. By FRANK HOWES, with a preface by Sir HUGH ALLEN. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1926. Pp. x + 244. 6*s.* net.

The region which the author has attempted to survey in this small volume is a fascinating one, but it bristles with difficulties, and these are increased when the public catered for is neither wholly musical nor wholly psychological.

The work will probably be of interest to musicians and to that large body of music lovers who are interested in psychology; but it contains little of value to the psychologist. The treatment of the various topics is much too elementary, while the outlook is too restricted and lacking in originality.

Besides an introductory chapter on Psychology and Music, there are six essays on Gregariousness in Audiences and Performers; Emotion in Music; Rhythm; Applause; Inspiration and the Sub-conscious; and Taste.

Influence of Music on Behaviour. By CHARLES M. DISERENS. Princeton University Press. (London: Humphrey Milford. Oxford University Press.) 1926. Pp. ii + 224. 11*s.* 6*d.* net.

The first six chapters of this work give an interesting, largely descriptive, account of the effects of music on animals and men—the influence of music on the sick and on work; its place in Myth and Magic. Then follows a chapter, of thirty pages, in which previous experimental work on reactions to musical stimuli are well summarized, while the eighth chapter, of fifty pages, contains an account of the author's own researches, conducted between the spring of 1921 and May 1922. There is also a final chapter in which a very brief summary of the book is given.

The chief value of the book lies in the fact that it constitutes a good summary of the reputed and experimentally found effects of Music, and that it contains an (unnumbered) bibliography of about 260 references. On this account it is unfortunate that there is no table of contents, and that no reference is given, in the short index, to the numerous authors consulted. The utility of the volume would have been increased if these had not been omitted. Still the book is one which can be recommended to all who are interested in the psychological effects of music. It will be of especial use to those who are working on any of the numerous problems involved.

Elements of Mathematics. By D. CARADOG JONES, M.A. and G. W. DANIELS, M.A., M.Com. London: Hodder and Stoughton, Ltd. University Press of Liverpool, Ltd. 1926. Pp. viii + 240. 8*s.* 6*d.* net.

This book, we are informed in a preface, is designed for students who have had only a slender training in Mathematics, and who, in consequence, are afraid of symbols and statistics. In its small compass are four chapters on Algebra including Progressions, Binomial Expansion, Probability, etc., three on Co-ordinate Geometry including Straight-Line Equations, Equations of the Second Degree, Simple Conics, etc., six on the Calculus including Differential and Integral Calculus, Curve Fitting, etc., and three on Calculus applied to Analysis of Economic Theory.

The task of preparing the volume has been well done, as regards arrangement and subject matter. The book will be of value to those who have a fair mathematical knowledge which they desire to revive; but to those with no knowledge of Mathematics beyond the "matriculation standard" the book will be of doubtful value, unless they have considerable mathematical aptitude, largely because there are no examples for the student to work out.

A Manual of Normal Physical Signs. By WYNDHAM B. BLANTON. St Louis: The C.V. Mosley Co. 1926. Pp. 215. $ 2.50.

This is a text-book, or reference book, intended for the general medical student. As Dr Blanton says: "In the text-books of Physical Diagnosis normal signs are so intermingled with pathologic signs that the average beginner in this subject finds himself hopelessly at sea in attempting to discover a clear description of just the normal." The book is purposely sketchy and arranged as a note-book to be amplified by class-room instruction. No doubt it may be of considerable use to some medical students.

Practical Psychology: Human Nature in Everyday Life. By E. S. ROBINSON. New York: The MacMillan Co. 1926. Pp. viii + 479.

This book is intended for students who take a short course in psychology and who do not intend to proceed to further detailed study, or to the acquisition of expert knowledge of the subject. A practical psychology is defined as "one in which scientific principles are given more emphasis than technical devices,...in which these principles are selected and treated in such a way as to bring out their intimate relations to the personal and social life of the student." On the whole the book is a very good one, having regard to its aims. It is clearly expressed and very well arranged, and it is comprehensive.

The Nation's Schools, their Task and their Importance. By H. BOMPAS SMITH. London: Longmans Green and Co., Ltd. 1927. Pp. viii + 308. 6*s*. net.

The book is a general discussion, closely in touch with the actual facts, however, of the functions of education in the life of the nation. It contains four parts, dealing with: The School's Task; The Nation as Educator; The Schools and their Work; and Some Things the School should Teach. Prof. Smith writes clearly, with a full knowledge of the relevant literature, and with sympathy and idealism. He makes a great many valuable suggestions with regard to the general lines of desirable advance in the organization of education in this country. The book displays great qualities of common sense and benevolence.

Notes on the Originality of Thought: The Concept of Internal Necessity: Poetic Thought and Constructive Thought. By LEONE VIVANTE. Translated by Professor BRODERICK-BULLOCK. London: John Lane, The Bodley Head, Ltd. 1927. Pp. 227. 7*s*. 6*d*. net.

The book consists of a number of statements, most of them cast into a somewhat dogmatic form, upon all manner of questions relating to logic, psychology, art and metaphysics. Many of these are interesting, and display considerable insight; many more are exceedingly obscure.

Apollonius, or the Future of Scientific Research. By E. N. BENNETT. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1927. Pp. 95. 2*s*. 6*d*. net.

This brief essay contains warnings against fraud combined with a somewhat uncritical acceptance of certain reported results of experiment, a good short account of contemporary lines of investigation, and an expression of hopes for the future.

Indian Journal of Psychology. Vol. I. Nos. 3 and 4. Calcutta University Press. 1926. Two rupees, p.a.

We are glad to note the publication of this new Journal of Psychology. It has a distinguished Board of Editors, who are acting for the recently formed Indian Psychological Association, and is issued quarterly. These two numbers contain interesting articles on a variety of subjects, and show a very definite experimental bent. The venture deserves and should obtain widespread support.

Das Recht der Dschagga. Von BRUNO GUTMAN. München: C. H. Beck'sche. 1926. S. 778. M. 30.

This enormous monograph contains a rich mine of information, of both psychological and ethnological interest, concerning the laws and legal customs and practices of a considerable group of East African natives. The volume is one of the well-known series edited by Professor Felix Krueger under the general title of "Studies in Development Psychology" (*Entwicklungspsychologie*). The discussion is arranged under the following headings: Rules of Blood Organization; Rules of Property Organization; Rules of Social Ranking; Offences against the General Good; Legal Proceedings and Trials; and Legal Pronouncements and the Individual. There is a relatively short but very interesting "afterword" by the Editor, on the Developmental Psychology of Law.

Le Peyotl: la Plante qui fait les yeux émerveillés. Par A. ROUHIER. Paris: Gaston Doin et Cie. 1927. Pp. xii + 371. 25 fr.

This is an absorbingly interesting and comprehensive study of the Mexican drug *peyote* which has, in one way and another, played an important part in the social life of many peoples. Dr Rouhier deals first with the botanical study of the drug, discussing the distribution of the *peyote*, its internal morphology and its histology. The second part of the book is concerned with a historical and ethnological treatment with particular reference to the part played by the *peyote* in certain more or less religious ceremonies of Mexico and the United States. An admirable account is given of the varieties of *peyote* cult as they have existed in the New World. The third part deals with the chemistry, pharmacology and possible therapeutic uses of the drug. Here there is a most interesting chapter on *L'ivresse peyotlique*, which contains a great many important psychological observations. The book is extraordinarily well documented, and has a comprehensive bibliography. Naturally, since it covers a vast amount of ground, the treatment is not everywhere complete, and one would, in particular, like to see a good many more original experiments and a more radical discussion of the effects of *peyote* upon image processes. But it is a fine book, excellently produced and with some beautiful plates.

Einführung in die Entwicklungspsychologie. Von HEINZ WERNER. Leipzig: Johann Ambrosius Barth. 1926. S. vii + 360. M. 16.50; geb. 18.60.

This very learned, but attractive book expounds what the author regards as a new point of view in 'developmental' psychology. Instead of studying the development of special social products and seeking to deduce psychological principles from these, he endeavours to compare individual psychological development in various special fields and thus to establish general psychological laws. While in general development is characterized throughout by increasing capacity for differentiation, there grows up also the tendency to build more centralized and closely knit structures in mental life. The book deals with a great variety of problems, from perception to the formation of concepts and the growth of personality, and treats both normal and abnormal processes. Dr Werner has certainly produced a book that merits the most careful and critical study.

Intentionalität, Reaktionität und Schwachsinn. Von HEINRICH NÖLL. Halle a. S. 1926. Carl Marhold. S. 36. R.M. 1.00.

This is a treatment in general terms of the development and significance of objective references and reactions with particular reference to the mental processes of the feeble-minded. Some discussion is added of the deductions from the general theories put forward which bear upon school practice.

Die Persönlichkeit des dreijährigen Kindes. Von ELSA KÖHLER. Leipzig. 1926. S. Hirzel. S. ix + 240. M. 13; geb. M. 15.

This is an admirable and comprehensive study, carried out under the direction of Professor Buhler, of the general mental status and processes of the three-year old child. As a record of original observation, well planned and described, it is of great value.

Die Geisteskrankheiten einschliesslich des Schwachsinns und die Psychopathischen Konstitutionen im Kindesalter. Von TH. ZIEHEN. Berlin. 1926. Von Reuther u. Reichard. S. ix + 554. M. 26.

Professor Ziehen has here revised and enlarged his well-known text-book, bringing his treatment fully up to date. The treatise is comprehensive and rests not only upon a wealth of original observations, but also upon a wide knowledge of the relevant literature to which extensive references are made.

Die Psychischen Störungen der männlichen Potenz, ihre Tragweite und ihre Behandlung. Von DR MAXIM-STEINER. Leipzig und Wien. Franz Deuticke. 1926. S. 59. G.M. 2.40.

This is a fourth edition. The author deals with organic and psychical symptoms, classifies the varieties of psychical impotence into four groups, discusses their etiology, their diagnosis and prognosis, the individual and social implications and range of impotency, and adds a few illustrative cases. There is a brief foreword by Prof. Freud.

Die Persönlichkeitsanalyse, eine vorbereitende Methode für jede Psychotherapie der Neurosen. Von DR ERNST JOLOWICZ. Leipzig: Georg Thieme. 1926. S. 83. M. 2.10.

The book is a clearly written and interesting monograph setting forth the importance of a careful historical and analytical study of personality as a preliminary to therapeutic treatment of psychical disorders. The author deals particularly with the Oedipus complex, with the play impulse, with school influences, with cultural settings, with sexual development, and with dreams and their interpretations.

Strindberg und Van Gogh. Von DR KARL JASPERS. Berlin: Julius Springer. 1926. S. 151. G.M. 6.

The author gives an interesting 'pathobiography' of Strindberg; then, dealing similarly, but less fully with Swedenborg and Holderlin, and, more fully again, with Van Gogh, strives to show the influence of *dementia praecox* in the productive works of them all.

Grundzüge der ärztlichen Psychologie in der täglichen Praxis. Von DR HEINZ FENDEL. Berlin: Urban u. Schwarzenberg. 1925. S. 134. M. 3.

A useful brief text-book in which most of the current methods of treatment receive some notice. The book is, however, rather overcrowded with references in view of its size.

Das Märchen und die Phantasie des Kindes. Von Dr CHARLOTTE BÜHLER. Leipzig: J. A. Barth. 1925. S. 84. R.M. 3.20.

Dr Charlotte Buhler has here produced an extremely interesting monograph well worthy of consultation by all students of child and social psychology. She deals in particular with the characters of the stories, the general setting of the stories, the activities dealt with in the stories and their dramatic representation, and adds some suggestive notes on the phantasies of thinking and intuition. The brief bibliography contains only German and two French titles.

De l'Habitude. Par FÉLIX RAVAISSON. Paris: Félix Alcan. 1927. Pp. xxxv + 62. 12 fr.

This is a reprint of Ravaisson's essay first published in 1838. M. Jean Baruzi has contributed a long introduction in which he deals in particular with the relations between Ravaisson and Schelling. He has had access to a number of unpublished manuscripts.

Further Contributions to the Theory and Technique of Psycho-Analysis. By SÁNDOR FERENCZI; compiled by JOHN RICKMAN. Authorized translation from the German by JANE ISABEL SUTTIE and others. London: The Hogarth Press. 1926. Pp. 473. 28*s.*

This important book makes number XI in the International Psycho-Analytical library under the general editorship of Dr Ernest Jones. The material, much of which is of absorbing interest, not only to the psycho-analyst but also to the general psychologist, is of rather diversified character, and is grouped under headings as follows: Nosology; Technique; Sexual Theory; From the Nursery—Short Papers; Dreams, etc.; Symbolism—Short Papers; Applied Psycho-Analysis; Medical Jurisprudence and Religion. At the end there is a bibliography of Ferenczi's writings up to and including the year 1926. A good many of the notes printed are rather slight, but the longer studies in particular merit the careful attention of all psychologists who wish to understand the nature and justification of the contribution made by psycho-analysis to theories of human behaviour.

The Mind and Its Mechanism. By PAUL and W. R. BOUSFIELD. London. 1926. Kegan Paul, Trench, Trübner and Co., Ltd. Pp. vii + 224. 9*s.* net.

These authors draw their chief inspiration from McDougall. They are very anxious to escape from a materialistic explanation of 'mental' phenomena, but their own solution is itself a materialism only very thinly veiled over, for they invoke the aid of a psychical structure conceived on close analogy to that familiar in modern physics. In spite of the fact that their psychical electrons may have a small 'mass' they are stoutly claimed to be 'immaterial.' It is as difficult to explain mental things by 'a psychic organ' composed of these particles, as to explain them by a really materialistic brain. The familiar psychological problems are peppered and salted to taste with the help of a plausible terminology and then regarded as explained. As an imaginative speculation the book is most interesting.

Social Differentiation. By CECIL CLARE NORTH, Ph.D. The University of North Carolina Press. London: Humphrey Milford, Oxford University Press. Pp. ix + 343. 11*s.* 6*d.* net.

This book is very well written in an interesting and readable style, and claims attention as a painstaking study of a wide range of facts in sociology. One feels, however, a definite lack of any unifying scheme of reconstruction. The author's ready optimism facilitates a change of outlook in the face of almost every succeeding problem. And although this is an advantage in some respects as it enables one to see the good side of everything, yet it is sometimes better in writing a book to enforce a constant point of view even if this is sometimes wrong.

The Psychology of the Movements of Handwriting. Translated and edited from the works of J. Crépieux-Jamin. By L. K. Gwin-Wilson. London: Geo. Routledge and Sons, Ltd. 1926. Pp. x + 196. 3*s.* 6*d.* net.

This work presents a brief summary of the works of M. Crépieux-Jamin, who by "relating all the small signs to the largest species of movements in handwriting" claims to have made possible for the first time "a really scientific study of handwriting" in its bearing upon character. Abundant illustrations are given.

The Story Behind the Gospels. By Bernard H. Allen. London: Methuen and Co., Ltd. 1926. Pp. 1–122. 3*s.* 6*d.* net.

This is an interesting attempt to write a kind of realistic psychological study of Jesus. It is well written and well informed.

PSYCHOLOGICAL ABSTRACTS

The American Psychological Association began the publication of its new journal, *Psychological Abstracts*, with the January 1927 number. It is edited by Prof. Walter S. Hunter of Clark University, assisted by the following co-operating editors from foreign countries: F. C. Bartlett, Cambridge University, V. M. Bekhterev, Leningrad, Ed. Claparède, University of Geneva, G. C. Ferrari, University of Bologna, A. Michotte, University of Louvain, H. Piéron, Sorbonne, M. L. Reymert (formerly) University of Oslo, and W. Wirth, University of Leipzig.

The journal is international in character. It consists of non-critical abstracts of articles and books on psychological and cognate subjects, which appear as soon as possible after the original publication. These abstracts are prepared by competent psychologists in America and Europe. They are published in English, and average one hundred and fifty words each.

The scope of *Psychological Abstracts* is indicated by the accompanying classification of its material: General Topics in Psychology, Sensation and Perception, Feeling and Emotion, Attention, Memory and Thought, Nervous System, Motor Phenomena and Action, Plant and Animal Behaviour, Evolution and Heredity, Special Mental Conditions, Nervous and Mental Disorders, Social Functions of the Individual, Industrial and Personnel Problems, Childhood and Adolescence, Educational Psychology, Biometry and Statistics, and Mental Tests.

It is the intention of the Association to make *Psychological Abstracts* invaluable, not only to psychologists throughout the world, but also to all persons who are working in allied fields, such as physiology, psychiatry, education and biology.

Sample copies of the January and February issues will be sent for examination on request. The annual subscription rate is $ 6.00 (overseas $6.25), cheques to be made payable to *Psychological Abstracts*. Subscriptions should be sent to H. S. Langfeld, Business Editor, Princeton University, Princeton, New Jersey.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

April 30, 1927. (With the Cambridge Psychological Society.)

Experiments on Learning to Perform Skilled Movements, by A. Michotte.

The Relevance of Visual Images to the Process of Thinking, by T. H. Pear, F. Aveling, and F. C. Bartlett.

Demonstrations:
- Long spells of skilled work, by K. G. Pollock.
- An experiment upon divided attention, by E. J. Lindgren.
- Hand and arm movements, by N. Davis and A. Blair.
- Peripheral Vision, by M. Salaman.
- Constructive Imagination, by E. Hutchinson.
- Illusory Movement, by R. de Silva.
- Experiments on Reading, by R. W. Pickford.
- Visual Effects of Flicker, by D. Buchanan.
- Skill in Shooting, by H. Banister.
- Localization of Sound, by P. E. Vernon.
- Plateaux in a Leanring Curve, by M. Drury Smith.

May 25, 1927. The influence of Conation on Cognition, by E. H. Wild[1].

A Conative Criterion for the Discrimination of Types of Instinctive Behaviour, by A. C. Garnett.

SECTIONAL MEETINGS.

Aesthetics Section.

May 20, 1927. The Functions of the Critic, by Ursula Roberts.

June 9, 1927. The Psychology of Creative Effort, by E. D. Hutchinson.

Education Section.

May 9, 1927. The Function of the School for the Young Child, by Susan Isaacs.

Medical Section.

May 25, 1927. An Experiment at Fontainebleau—A Personal Anamnesis, by James Young.

June 13, 1927. The Adaptation of the Family to the Child, by Sandor Ferenczi.

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[1] In place of Dr Wild's paper advertised for Feb. 12 (see this *Journal*, vol. XVII, p. 383) a paper entitled "Adaptability to New Situations" was read by S. R. Laycock.

BRITISH PSYCHOLOGICAL SOCIETY

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[1] It is requested that notification of any errors in this list be sent to Miss M. L. St George, 121 A, Gloucester Road, South Kensington, S.W. 7.

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the Meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of members.

12. Every Candidate for election to the Society shall be recommended by at least two members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from Membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to sub-divide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall

immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, Chairman and Secretary of each of the Sections and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two ordinary members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of members to serve on the Council may be suggested by the Council or by any member by sending the proposed names to the secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council or by ordinary members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings, by ballot in accordance with Rule 15 at the next General Meeting, due notice having been given. Similar vacancies among the ordinary members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent members, to whom the resolutions adopted shall be forwarded.

29. The first meeting of each Special Section shall be convened by the General Secretary of the Society. At this meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional, and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall

be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or failing him a Vice-President, shall whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all members of the Society notices of each meeting at least seven days beforehand, and of the business to be transacted at the meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty members; at least fourteen days' notice of such meeting shall be sent to each member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any meeting. The Council or a Sectional Committee shall however have the power of closing any meeting or meetings to visitors, provided notice to this effect be given on the notice convening such meeting or meetings. The names of visitors together with the names of members introducing them shall be entered in a book provided for that purpose. Any member of the Society attending a meeting of a section to which he does not belong must be introduced by a member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of ordinary members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as ordinary members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

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LA PREMIÈRE ANNÉE DE L'ENFANT.

PAR J. PIAGET[1].

LA première année de l'enfant est encore, malheureusement, un abîme de mystères pour le psychologue. Si nous pouvions connaître les états de conscience du bébé, en même temps que nous observons ses mouvements, nous comprendrions sans doute toute la psychologie. Mais, malheureusement, cette connaissance nous sera toujours refusée. Certains auteurs, il est vrai, dédaignent de s'occuper de la conscience primitive et se limitent à l'étude des gestes. Mais, comme l'adulte a une pensée, comme l'enfant de tout âge a une pensée, et bien différente de la nôtre, il est nécessaire de se demander comment cette pensée a pu naître, et c'est donc de la genèse de la pensée chez le bébé qu'il faut nous occuper, même si c'est là un sujet singulièrement difficile.

Commençons par chercher une méthode. Nous voyons le bébé agir, gesticuler, saisir des objets et les mettre en mouvement. D'autre part, nous voudrions connaître ses pensées, et, pour cela nous cherchons, dans notre propre expérience adulte les états de conscience qui correspondent aux gestes que nous observons chez lui. Malheureusement, nous ne trouvons ainsi rien du tout, car d'une part il y a longtemps que nous ne faisons plus de tels gestes, et d'autre part nos états de conscience sont si complexes qu'en les projetant chez le bébé, nous avons le sentiment très justifié de dénaturer les siens. Donc, première règle de méthode: méfions-nous des comparaisons avec l'adulte, ou, si vous voulez, de l'"adulto-centrisme." —Heureusement que cette règle a sa contre-partie. Nous sommes, cela est vrai, des adultes. Mais, entre le bébé et nous, il y a tous les intermédiaires: il y a l'adolescent, l'enfant de 11 ans, l'enfant de 7 ans, et celui de 2 ans. Ces mentalités successives obéissent à des lois génétiques, et il n'est pas impossible de pénétrer en ces pensées diverses. Or, nous retrouvons, chez le petit enfant, beaucoup de résidus des conduites du bébé. Si nous arrivons, chez l'enfant, à reconstituer la signification de ces conduites et les états de conscience dont elles s'accompagnent, nous pouvons, par une méthode régressive, espérer introduire quelque lumière dans les ténèbres de la première année. Pour ma part, en observant ma fille pendant ses premiers mois, j'ai eu sans cesse l'impression de trouver la clef de phénomènes observés à l'état de simples vestiges dans la conduite et la pensée des enfants plus âgés. Assurément, la méthode est dangereuse. Mais c'est celle dont on use en biologie. Le biologiste compare sans cesse les animaux supérieurs aux animaux inférieurs et les uns s'éclairent par les autres. Il y a là une sorte de jeu de navette qui, manié avec prudence, délivre précisément des idées évolutionnistes trop audacieuses comme des illusions du fixisme.

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[1] Paper read before the British Psychological Society March 7, 1927.

Cela dit, cherchons une hypothèse de travail. En étudiant la pensée des enfants de 3 à 15 ans, nous sommes arrivés à la conclusion que le caractère le plus important de cette pensée—celui qui est en relation avec tous les autres—est ce qu'on peut appeler l'*égocentrisme* de l'enfant. Si nous passons à la limite, nous pouvons admettre que le bébé présente un égocentrisme absolu, que nous pouvons appeler le *solipsisme* de la première année. Nous allons chercher à expliquer les attitudes du bébé par le solipsisme, comme nous avons interprété la pensée de l'enfant par l'égocentrisme.

Le plan que nous allons suivre est donc simple. Dans une première partie nous allons comparer le solipsisme primitif à l'égocentrisme de l'enfant. Dans une seconde partie nous allons analyser à ce point de vue un exemple privilégié, celui de la causalité chez le bébé.

I. L'ÉGOCENTRISME DE L'ENFANT ET LE SOLIPSISME DU BÉBÉ.

Tout le monde a toujours dit que l'enfant est égocentrique. Qu'est-ce que cela signifie?

Presque tous les psychologues admettent aujourd'hui qu'il y a quelque chose de social dans la pensée logique. En nous forçant à parler, la société a complètement modifié notre intelligence. Avec l'aide des mots, la pensée devient conceptuelle, générale et abstraite. Sans les mots nous penserions seulement comme on fait en rêve: avec des images, groupées par des sentiments et chargées de signification affective, vague et toute individuelle, c.-à-d. d'une manière plastique. Grâce au langage, nous apprenons à discuter, et la discussion avec les autres donne naissance à cette discussion intérieure qu'on appelle la réflexion. C'est ainsi la société qui nous a appris à réfléchir. C'est encore elle qui nous a appris à prouver. La preuve est née, en effet, de la discussion. Quand nous pensons pour nous-mêmes, nous n'avons pas besoin de preuves. Nous nous croyons toujours tout de suite. Ce sont les autres, en nous contredisant, qui nous ont appris à être exigeants. Or la logique n'est autre chose que le système des preuves. La logique ne peut donc se développer, au point de vue psychologique, que dans un milieu social. La pensée adulte, la pensée logique, est donc une pensée socialisée. C'est là un caractère qui lui est constitutif et non surajouté.

C'est ce que la pathologie nous enseigne clairement. En étudiant la schizophrénie, M. Bleuler a montré que la pensée des malades est *autistique* parce que les malades ne cherchent plus l'objectivité, mais la satisfaction personnelle. Ils n'entrent plus dans la pensée des autres. Ils ne se soucient pas de ce que les autres pensent autrement qu'eux; ils cherchent simplement à vivre leur rêve personnel. Or c'est pour cela que leur pensée devient si étrange. La pensée autistique, qui comprend aussi la *pensée symbolique* décrite par Freud et les psychanalystes, est simplement ce que serait notre pensée individuelle à tous, si la société n'existait pas. Cette pensée ne procède pas par mots mais par images, par tableaux chargés d'affectivité. Elle ne se soucie pas d'objectivité, mais cherche uniquement la réalisation des désirs, par voie de jeu ou d'auto-illusion.

Bref, l'autisme et la pensée logique sont aux deux pôles de l'individuel pur et du social pur, et notre pensée adulte normale oscille sans cesse entre les deux pôles.

Qu'en est-il maintenant de l'égocentrisme enfantin? L'enfant égocentrique est très différent d'un schizophrène autistique, parce que, à l'opposé du malade, il se soucie beaucoup des autres et a tout le temps besoin de leur société. Le schizophrène, en effet, se désintéresse entièrement de l'opinion des autres. Il vit dans son moi et s'y complaît. Il se défend contre l'entourage. Il est négativiste. L'enfant, au contraire, n'a nullement l'impression de vivre dans son moi. Il croit penser comme tout le monde et s'y efforce sincèrement. D'autre part, par le fait même qu'il se retire de la société, l'autiste morbide ne s'intéresse plus à la réalité. Il vit dans son rêve et ne cherche plus l'adaptation aux choses. L'enfant, au contraire, tend, de toutes ses forces, vers le réel et vers l'adaptation. Il n'est donc pas a-social: il est pré-social.

Mais d'autre part, l'enfant est aussi très différent de l'être socialisé. Et il n'est pas purement autistique, il n'est cependant pas adapté à la pensée des autres. Il croit sans cesse comprendre les autres et être compris d'eux, mais quand on analyse les choses de près, on voit qu'il n'en est rien. On s'aperçoit qu'il déforme sans cesse la pensée des autres, parce qu'il y mêle la sienne sans le vouloir, et on découvre qu'il sait très mal exprimer aux autres sa pensée, parce qu'il a dans l'esprit toutes sortes d'images ou de schémas moteurs qui sont, en fait, personnels et inexprimables, mais qu'il considère comme commun à tous. Bref, l'enfant croit naturellement être en communion d'idées avec tout le monde et c'est pourquoi il est égocentrique: il croit que tout le monde pense comme lui, simplement parce que, à son insu, il n'est jamais sorti de lui-même.

Par conséquent, quoique l'enfant soit beaucoup plus socialisé qu'un pur autiste, la pensée enfantine reste encore autistique dans sa structure propre. On retrouve, en effet, dans la logique de l'enfant, tous les caractères de la logique autistique. Ainsi la pensée de l'enfant n'est pas conceptuelle, mais riche en images et en schémas individuels, en analogies inexprimables, en visions d'ensemble pénétrées de sentiments. Le raisonnement enfantin n'est pas déductif, général et rigoureux, mais consiste en enchaînements d'affirmations particulières sans cohérence formelle. Tout est croyance spontanée, affirmation sans contrôle. Il n'y a ainsi pas d'objectivité réelle, mais simplement l'illusion continuelle d'être en accord avec tout le monde. La causalité enfantine n'est qu'une projection du moi dans les choses.

Cela dit, nous pouvons passer à l'étude du bébé et de la première année du développement mental. Nous allons retrouver, si notre méthode est bonne, les deux mêmes paradoxes que dans la pensée de l'enfant, mais encore accentués.

Le premier paradoxe est celui du solipsisme du bébé. Le bébé paraît tout entier tourné vers le monde extérieur, vers le dehors. Il n'a très vraisemblablement aucune conscience de son moi. Il découvre son corps comme un corps extérieur. Néanmoins, si l'enfant est d'autant plus égocentrique qu'il est plus jeune, il faut bien admettre que le bébé l'est encore plus que l'enfant. Freud a parlé du narcissisme du bébé, comme si le bébé n'avait aucun intérêt pour autre chose que lui-même. Nous pouvons même faire l'hypothèse, et cela surtout en étudiant la naissance de la causalité, que le bébé prend l'ensemble de son univers pour son moi, à la manière du solipsiste qui identifie le monde avec sa représentation.

Second paradoxe. La pensée du bébé consiste en un ensemble de mouvements d'accommodations adaptés au monde extérieur. Néanmoins cette pensée rappelle à d'autres égards, une sorte de continuel rêve éveillé, avec tous les caractères de l'autisme intégral.

Commençons par le premier paradoxe, et cherchons à comprendre le solipsisme de la première année. Quand le bébé ouvre les yeux à la vie et fait peu à peu la conquête de l'univers, ce qu'il découvre en dernier lieu, ce qu'il ne soupçonne pas le moins du monde pendant les premiers mois de son existence, c'est sa propre personnalité. C'est là un résultat que J. M. Baldwin a établi d'une manière qui semble définitive, en analysant son fameux stade projectif. Il paraît bien, en effet, que la conscience du moi s'acquière par différenciations successives et que, durant les stades primitifs, tout le contenu de la conscience soit mis sur le même plan, sans qu'il y ait de monde intérieur et de monde extérieur qui s'opposent l'un à l'autre. Ainsi, vraisemblablement, quand le bébé est joyeux ou est triste, il colore tout son univers de sa joie et de sa tristesse; quand il mange ou qu'il suce, tout ce qu'il voit lui paraît participer de ses sensations de succion; et inversement, quand il découvre son propre corps, ses doigts, ses pieds, ses bras, il les considère exactement comme les autres objets, sans se douter que ces objets particuliers dont il admire les mouvements sont en fait mus par lui-même. Nous pouvons donc accorder à Baldwin que, à l'origine, le bébé n'a en aucune manière le sentiment de son moi.

Mais alors, une question troublante se pose, au sujet de la pensée du bébé et au sujet de la méthode régressive que nous comptions employer pour interpréter les faits révélés par l'observation des conduites primitives: si le bébé n'a pas le sentiment de son moi, peut-on parler du solipsisme du petit enfant, et peut-on, avec Freud, appeler narcissisme l'apparent amour du bébé pour sa propre personne? Il paraît, en effet, y avoir contradiction dans les termes à nier la conscience du moi et à parler de narcissisme ou de solipsisme. Si tel est le cas, peut-on légitimement comparer l'égocentrisme de l'enfant plus âgé à l'attitude du bébé qui ignore complètement son moi? Peut-on par conséquent chercher dans la conduite et dans la pensée du bébé les racines des grands phénomènes qui caractériseront plus tard la pensée de l'enfant égocentrique?

A cette question, je vous propose de répondre par l'affirmative. Je vous propose d'admettre le solipsisme du bébé et d'admettre qu'il constitue la source de l'égocentrisme enfantin, et nous verrons tout à l'heure qu'il y a là une bonne hypothèse de travail pour interpréter les faits que nous allons décrire. Il faut seulement nous entendre sur la signification de ces concepts.

Il est évident que si l'on entend par solipsisme l'attitude d'esprit d'un philosophe qui considère l'univers entier comme contenu dans son moi, il est absurde de dire que le bébé est solipsiste, puisque justement le bébé n'a pas la conscience de son moi. Seulement ce n'est pas cela du tout qu'est le solipsisme. Il n'y a pas de philosophes solipsistes, et ceux qui croient l'être se font illusion à eux-mêmes. Le vrai solipsiste se sent identique à l'univers, et tellement identique qu'il ne sent même pas la possibilité de deux termes. Le vrai solipsiste projette tous ses états de conscience dans les choses. Le vrai solipsiste est réellement seul au monde, c.-à-d. qu'il n'a aucune idée de quelque chose qui lui serait extérieur. Autrement dit, le vrai solipsiste n'a aucune notion de son moi. Il n'a pas de moi: il est le monde. Il n'y a donc aucune absurdité à considérer le bébé comme solipsiste, car lui seul est capable de l'être réellement. Dire que le bébé est solipsiste cela signifie donc simplement que les sentiments et les désirs du bébé n'ont pas de limites, mais font corps pour lui avec tout ce qu'il voit, tout ce qu'il touche, tout ce qu'il perçoit.

Il est evident, dans ce cas, que le bébé est narcissiste, mais il ne l'est pas comme un adulte, ni même comme le Dieu de Spinoza, et j'ai un peu peur que Freud oublie quelquefois que le bébé narcissiste n'a pas le sentiment de son moi.

Enfin, il est clair, si l'on admet cela, que l'égocentrisme enfantin est simplement le prolongement de ce solipsisme initial. Nous avons vu, en effet, que l'égocentrisme enfantin n'est pas un processus intentionnel ni même conscient. L'enfant ne sait pas qu'il est égocentrique. Il croit sa pensée universelle et cette fausse universalité provient simplement de l'absence du sentiment des limites de son individualité. S'il en est ainsi l'égocentrisme est entièrement comparable au solipsisme: tous deux proviennent de l'absence ou de la faiblesse de la conscience du moi.

Il faut donc nous attendre à ce que nous trouvions chez le bébé, et en beaucoup plus accentués, les traits principaux de la pensée égocentrique. Ceci nous conduit à l'examen du second paradoxe dont nous voulions parler. L'enfant égocentrique est à la fois plus empiriste que nous et plus dominé par la logique autistique. Il faut donc nous attendre à trouver chez le bébé solipsiste un mélange *sui generis* d'accommodation par pur tâtonnement et d'autisme pur, quelque chose comme l'intelligence toute empirique de l'animal unie à une sorte de rêve éveillé continuel. C'est ce que nous allons chercher à montrer maintenant.

Si vraiment le bébé n'a pas la conscience de son moi, et est entièrement tourné vers les choses, mais que, d'autre part, tout son moi, tous ses états de conscience, sont complètement projetés dans les choses, notre second paradoxe devient clair par cela même: d'une part, la pensée du bébé consistera en accommodation pure ou en tâtonnements moteurs, mais, d'autre part, cette même pensée ne constituera qu'un long rêve éveillé, qu'un autisme intégral.

Commençons par le tâtonnement moteur. Il est bien connu et nous n'y insisterons pas. Il y a d'abord ce que Baldwin a appelé la *réaction circulaire,* et qui est le point de départ de toutes les accommodations.

L'enfant gesticule au hasard et, lorsque quelque résultat intéressant s'est produit, il répète le geste indéfiniment. C'est ainsi qu'il apprend à sucer son pouce, à saisir les objets, à produire des sons en frappant sur des corps durs, etc. La réaction circulaire, c'est donc l'utilisation du hasard. En second lieu, le bébé apprend à imiter, grâce à des processus que nous ne voulons pas étudier ici, et cette imitation joue un grand rôle dans l'accommodation motrice de l'enfant. Nous pensons même que c'est l'imitation des personnes et des objets inertes qui donne à l'enfant la possibilité de construire des images mentales. L'image mentale est le produit d'une imitation intériorisée, comme la réflexion est le produit d'un langage intériorisé. L'accommodation motrice et l'imitation-image se prolongent ensuite en mémoire, et l'on sait combien extraordinaire est la mémoire des petits, dès la fin de la première année. Bref, et sans qu'il soit besoin d'insister, il y a là un ensemble de démarches caractérisant le premier aspect de la pensée primitive: le bébé pense par accommodations motrices et mentales qui lui fournissent, grâce à l'imitation de soi-même, à l'imitation d'autrui et à l'imitation intériorisée, un ensemble de copies plus ou moins directes des choses. Ce sont ces matériaux qu'utilise l'intelligence empirique lors de sa naissance.

Seulement cette accommodation du bébé aux choses, que l'on a coutume de considérer comme définissant toute la pensée naissante, n'en constitue en réalité qu'un aspect. Un second aspect est tout aussi essentiel: on peut considérer la pensée du bébé comme une sorte de rêve éveillé, c.-à-d. comme le point de départ de l'autisme. Et le paradoxe de la pensée naissante est précisément que les deux aspects si contraires sont en pratique intimement mêlés.

Voyons les faits. Les trois principaux caractères de l'autisme en général, ou du rêve en particulier, nous paraissent être: 1^o l'assimilation du monde au moi, 2^o la formation de schémas affectifs et imagés, et 3^o la direction spéciale de la pensée, par association affective et non par systématisation logique. Nous allons voir que ce sont là justement les caractères principaux de la pensée solipsiste du bébé.

En premier lieu l'*assimilation*. Le propre de l'intelligence, c'est l'adaptation de l'esprit aux choses, c'est une sorte d'accommodation intellectuelle préparée par l'accommodation motrice dont nous venons de parler. L'autisme ou le rêve, au contraire, adaptent les choses à notre désir, sans aucun souci d'objectivité. Or il est frappant de constater que c'est là uniquement ce que fait le bébé, et que chez lui, l'accommodation est entièrement soumise, au début, à une sorte d'assimilation perpétuelle du monde extérieur au moi.

En effet, dès son arrivée au monde, le nouveau-né éprouve vraisemblablement un certain nombre d'états de conscience liés à son activité réflexe initiale. Ce sont les sensations gustatives liées à la succion, les sensations tactiles liées également aux réflexes de succion ou à ceux de la main, les sensations visuelles, auditives, etc. Or on peut dire que la loi fondamentale de l'activité psychologique naissante est une recherche de la conservation et de la reproduction des états de conscience intéressants.

Or, tout naturellement, le bébé découvre, dans cette activité, une foule d'éléments nouveaux. La recherche du sein maternel n'aboutit pas nécessairement au succès: c'est sa main, son oreiller, sa couverture ou n'importe quoi, que l'enfant heurte par hasard de la bouche. C'est alors qu'apparaît l'assimilation du monde au moi qui est si caractéristique de l'activité psychique à ses débuts. De même que l'organisme transforme le milieu extérieur pour l'assimiler à sa propre substance, de même la conscience naissante transforme tous les éléments nouveaux qui surgissent, pour les incorporer à ses propres tendances: le corps étranger qui heurte la bouche du bébé n'intéresse nullement en lui-même la conscience, il est considéré simplement comme quelque chose qu'on peut sucer et qui déclenche les sensations de succion. Il est ainsi assimilé par le schéma de la succion.

C'est ainsi que tout devient matière à assimilation. L'ensemble de l'univers du nouveau-né se répartit en: choses à sucer, choses à saisir, choses à regarder, choses à écouter, etc. Or, il y a là quelque chose de tout opposé à l'intelligence et d'identique au rêve. Quand nous rêvons d'un pays éloigné que nous traversons en voyage, etc., la fonction de la pensée n'est pas de nous apprendre quelque chose sur ce pays, mais simplement de réaliser un désir: désir de grandeur, désir sexuel ou tout ce qu'on voudra. Et le rêve dénature tout pour réaliser ces désirs. La pensée primitive fait de même.

Or cette attitude de continuelle assimilation, qui est bien claire durant les premiers mois, dure en réalité très longtemps chez le petit enfant. Que l'on observe, par exemple, un bébé de 8 mois en face d'un objet nouveau. Je présente à ma fille (8 mois 16 jours) un porte-cigarette. Elle le prend des deux mains, le regarde et fait *pff, pff*, ce qu'elle dit en général aux personnes, qui se mettent alors à rire. Puis elle le frotte contre le bord de son berceau (pour produire un son). Puis, elle le regarde à nouveau et se cambre (se cambrer est chez elle un geste magique, que nous étudierons à propos de la causalité, et qui lui sert à mettre les choses en mouvement quand elle ne peut pas les atteindre directement). Ensuite elle le balance au-dessus d'elle et enfin elle le met dans sa bouche. Donc en face d'un objet nouveau, la petite essaye de l'incorporer à cinq schémas différents d'assimilation. C'est l'attitude constante du bébé. Après avoir classé les choses en fonction des réflexes élémentaires, ce qui constitue l'assimilation psychique à l'état fruste, il a appris, par combinaison de ces cycles fondamentaux d'assimilation, pour ainsi dire, à se construire des schémas nouveaux: par exemple, saisir les objets pour les mettre à la bouche, secouer pour produire un son, frotter, etc., et il passe son temps et ses journées à faire rentrer l'univers entier dans ces nouveaux schémas.

Assurément, cela n'est pas de la pure assimilation, puisqu'il y a sans cesse apprentissage et accommodation. Mais c'est là, justement, qu'est le paradoxe et le problème: à chaque instant la réalité fait craquer les cadres des schémas d'assimilation et provoque de nouvelles adaptations, mais l'assimilation prend sa revanche et rétablit sans cesse la continuité entre le nouveau et l'ancien.

Passons au second point: les schémas affectifs et imagés. L'intelligence procède par concepts, qui sont tout d'abord des généralisations d'expériences motrices, pour finir dans la relation pure, grâce à l'abstraction progressive. L'autisme, au contraire, procède par condensation d'images sous l'influence du sentiment. Or, on trouve évidemment chez le bébé, des embryons de concepts dans les schémas d'accommodation. Mais on trouve tout autant, et intimement unis à eux, ce qu'on peut appeler des *symboles* (la pensée symbolique étant conçue comme le contraire de la pensée par "signes," c.-à-d. du langage et de l'intelligence). Les symboles autistiques sont caractéristiques du jeu des petits enfants et dérivent très directement des schémas d'assimilation dont nous parlions à l'instant.

Il est assurément très difficile de dire quand commence le jeu, chez l'enfant, mais il est probable que la question n'a pas de sens, car l'assimilation que nous venons de décrire peut être considérée comme le point de départ du jeu lui-même. Ce qu'on peut se demander, par contre, c'est à quel moment le bébé imagine, devant un objet, qu'il en a un autre à la place. Cette attitude est très facile à observer dès que l'enfant parle. Ainsi, à 1 an et 9 mois déjà, ma fille pousse devant elle une boîte en disant "Automobile," ou fait semblant de manger du papier et dit "Très bon." Deux mois auparavant, elle ordonnait à son chien de pleurer et faisait elle-même la mimique des larmes. Or, notons le, de telles conduites constituent simplement le prolongement de l'assimilation. Qu'un enfant, avant de savoir parler, se serve d'un objet quelconque pour satisfaire son besoin de succion, ou de préhension, etc., ou que, sachant parler, il se serve d'une boîte pour se représenter l'automobile qu'il a envie de voir, le processus est exactement le même: l'objet n'est pas conçu en lui-même, mais en fonction du désir. Seulement, dès que l'enfant sait parler, nous avons la preuve que ces assimilations aboutissent bien à la formation de symboles analogues à ceux de la pensée autistique en général.

En effet, l'analogie du rêve et du jeu des enfants a été bien souvent soulignée. Quand une petite fille de 22 mois, par exemple, prend un coquillage, lui fait faire dodo, lui donne des soins de toilette, etc., le coquillage devient ainsi symbole d'un groupe de sentiments et d'intérêts et sa seule différence avec un symbole onirique est qu'il est beaucoup plus conscient. Mais, comme on sait, on trouve dans la pensée autistique tous les degrés de conscience entre le symbole transparent du jeu ou de l'art, et les symboles obscurs du rêve ou de l'aliénation. L'essentiel, pour nous, est de marquer ici que cette formation du symbole est prédéterminée dès les habitudes mentales les plus primitives que nous venons de décrire sous le terme d'assimilation.

En troisième lieu, il est bien facile de montrer que la pensée du bébé est, comme on dit, "non dirigée," c.-à-d. que sa direction est analogue à celle d'une rêverie profonde, et non à celle d'une intelligence qui tend vers un but et qui subordonne toute ses démarches à la poursuite de ce but. Voici, par exemple, un bébé de 9 mois qui cherche à atteindre un objet suspendu au toit de son berceau. Il fait des mouvements dirigés dans ce but, mais voyant le toit s'ébranler, il oublie son but primitif, et ne cherche plus qu'à secouer tout le berceau. Puis il revient au but primitif, tire une ficelle, mais, en cours de route, remarque ses mains. Il ne s'occupe plus alors que d'elles et les agite. Ce mouvement lui donne l'idée de prendre son oreiller et de l'agiter aussi en l'air. Mais il remarque les franges du bord de l'oreiller et se met à les sucer. Ce geste lui rappelle la position du sommeil et il se met alors aussitôt dans cette position, etc., etc. On voit ainsi que la pensée du bébé consiste en une série de cycles schématisés de mouvements, cycles qui s'accrochent et se déclenchent les uns les autres par association. Chaque cycle comporte bien un but, et des moyens pour y parvenir, et en ce sens il y a des directions partielles dans la pensée. Mais ces cycles ne sont nullement hiérarchisés les uns par rapport aux autres. Ils se succèdent simplement, au gré des hasards et des assimilations.

Il est inutile de poursuivre plus longtemps le parallèle entre la pensée du bébé et la pensée autistique en général. On constate ainsi que, quoique la conduite du bébé consiste en une perpétuelle accommodation motrice aux choses, l'ensemble n'en constitue pas moins une sorte de long rêve éveillé. Le bébé est donc à la fois beaucoup plus près et beaucoup plus loin des choses que nous. Or, si nous reprenons notre hypothèse du solipsisme initial, ce paradoxe devient intelligible. L'essence de la pensée primitive consiste, en effet, en ce que le moi et le monde extérieur ne font qu'un et ne sont aucunement différenciés. Pour le bébé il n'y a donc pas de choses en dehors de son moi, ou, si l'on préfère, l'ensemble des désirs et des sentiments sont projetés dans les choses. Dès lors, l'ensemble des choses sont assimilées aux désirs et aux tendances subjectives. Cette adaptation des choses au moi suppose assurément une continuelle accommodation motrice, et c'est pourquoi le bébé apprend sans cesse de nouvelles conduites au lieu de piétiner sur place. Mais, ce qu'il cherche constamment, c'est néanmoins de faire rentrer le nouveau dans l'ancien, et d'assimiler l'univers entier aux exigences de son moi.

Mais tout cela doit paraître terriblement schématique, tant que l'on n'entre pas dans le concret des faits. Pour préciser ces hypothèses nous allons maintenant analyser un domaine particulier de l'activité de la première année: le domaine de la causalité. Si nos déductions sont exactes, nous devrons trouver deux résultats au moins. 1°. La seule forme de causalité qui convienne à une pensée solipsiste, c'est une sorte de magie primitive telle que le désir, accompagné du geste, soit censé agir sur la réalité. 2°. Mais si vraiment le bébé est à la fois plus près et plus loin des choses que nous-mêmes, cette magie ne pourra naître qu'à l'occasion des rapprochements fortuits créés par l'expérience la plus empirique et la moins rationnelle.

C'est ce que nous allons chercher à établir.

II. La causalité primitive.

Il est facile de voir que, chez l'enfant égocentrique, entre 3 et 8 ans en particulier, la causalité consiste en une sorte de perpétuelle assimilation des phénomènes physiques aux processus donnés dans l'expérience psychologique. Expliquer, pour l'enfant, c'est supposer, derrière le phénomène, une intention calquée sur nos intentions et une force de réalisation calquée sur les pouvoirs de la personne humaine. La causalité enfantine, ou précausalité, est donc directement issue de l'égocentrisme.

Qu'en sera-t-il du bébé solipsiste. Pour le comprendre, cherchons une fois de plus à passer à la limite de l'égocentrisme. Le bébé n'ayant pas, par hypothèse, la conscience de son moi, il ne pourra concevoir les choses comme des êtres distincts les uns des autres et semi-personnels.

Il y aura continuité totale entre l'interne et l'externe. Chaque phénomène perçu sera tout à la fois chargé de qualités physiques (goût, résistance, son, couleur, forme, etc.) et chargé de qualités psychiques (plaisir ou peine, désir, sentiments d'effort, d'efficace, de résistance, d'attente, d'inquiétude, etc.). Bref, tout mouvement extérieur apparaîtra comme le prolongement de sensations kinesthésiques et de sentiments. Il s'établira ainsi des liaisons causales que l'on ne saurait encore classer dans les catégories de la causalité enfantine (explications intentionalistes, animistes, artificialistes, dynamistes, etc.), mais que l'on pourrait très grossièrement comparer à une sorte de magie, et que nous appellerons la *causalité par efficace*: l'essence en est une liaison entre les sensations corporelles (ou les sentiments élémentaires) et les mouvements du monde extérieur, et cela sans aucun contact spatial ni aucune connexion intelligible.

Mais il va de soi que ces liaisons par efficace ne vont pas tomber du ciel dans l'esprit de l'enfant. C'est toujours à l'occasion d'un rapprochement fortuit créé par l'expérience que ces liaisons prendront naissance. Du point de vue externe, du point de vue de la pure réaction, la causalité primitive paraît donc *phénoméniste*. Mais, du point de vue de la psychologie réflexive nous pouvons considérer comme très probable que les liaisons phénoménistes s'accompagnent de sentiments d'efficace, car nous voyons de nombreux restes de ces conduites dans la deuxième, la troisième, la quatrième année, et même au delà, et il nous est alors possible de les analyser directement. Quant à la première année, la mimique de l'enfant ne laisse presqu'aucun doute sur le sentiment qu'il a d'être cause et sur le plaisir qu'il en retire.

Passons aux faits eux-mêmes. La première expérience que le bébé fait de la causalité est sans contredit à chercher dans ce que Baldwin a appelé la *réaction circulaire* dont nous avons déjà parlé à un autre point de vue: l'enfant fait par hasard un mouvement, prend plaisir à son résultat, et recommence indéfiniment comme pour reproduire le même résultat (que ce désir de reproduction soit une conséquence ou une cause de l'acte reproduit, peu importe). Par exemple, vers la fin du second mois et au courant du troisième, le bébé se met à glisser son pouce dans sa bouche, non plus par hasard mais systématiquement, et à le sucer sans cesse. Durant son 4ème mois, j'ai vu ma fille regarder longuement les mouvements de ses doigts, qu'elle répétait constamment. A 6 mois, elle s'est mise à saisir les objets et à les apporter dans son champ visuel. A 8 mois elle regardait sans fin ses mains s'ouvrir et se fermer. Au même âge elle prenait grand plaisir à observer les mouvements de ses pieds, etc., etc.

Il est assurément très dangereux de vouloir deviner ce que pense un bébé qui regarde ses mains et ses pieds tout en les faisant remuer doucement. Mais à ce même âge le bébé, comme nous allons le voir, fait des choses si extraordinaires, pour agir sur le monde extérieur, qu'il nous faut bien tâcher de débrouiller la signification de ses actes élémentaires. Nous verrons, par exemple, la même petite fille, et aussi à l'âge de 8 mois, remuer sa main pour faire bouger les objets à distance, ou se soulever sur ses omoplates et retomber de tout son poids pour mettre en mouvement les choses qu'elle voit au loin. Nous verrons un bébé à peine plus âgé cligner des yeux devant le cordon d'une lampe pour faire allumer la lampe, etc. Si l'on ne veut pas construire toute la psychologie avec les réflexes conditionnés, comme on faisait autrefois avec les associations d'idées, il convient de chercher à comprendre. Revenons donc au bébé qui regarde remuer ses mains et ses pieds, et essayons de nous mettre à sa place en oubliant notre mentalité.

Deux choses au moins paraissent claires dans sa conduite. D'une part, il désire que le mouvement observé continue de se produire, et fait effort pour que cela continue. Il n'y a rien là de mystérieux (sinon dans les rapports du désir et de l'acte). Mais, d'autre part, il est très étonné de ce qu'il voit, et il regarde son propre corps comme nous regardons un animal que nous ne connaissons pas. Or, évidemment, il faudrait un vrai miracle pour que le bébé se dise: "Cette main, c'est à moi; mon désir de la voir remuer, c'est encore à moi; c'est donc moi qui me fais remuer moi-même." Il est infiniment plus probable, étant donné ce que nous savons des difficultés de la pensée de l'enfant plus âgé, comme, en général, des rapports entre la conscience et l'action, que le bébé devant sa main n'éprouve aucun sentiment de ce genre, mais quelque chose de beaucoup plus simple. Admettons, par prudence, que les premières répétitions de l'acte ne s'accompagnent d'aucun état de conscience. Il faut bien supposer, cependant, que tôt ou tard, la tendance à la répétition se traduira dans la conscience sous forme de désir ou de sensation d'effort, si vagues soient-ils et dépourvus de toute représentation du but à atteindre. Or, par le fait même de la répétition, ce désir ou cet effort seront forcément encastrés entre deux perceptions visuelles du mouvement de la main, sans que l'enfant conçoive aucun intermédiaire entre le sentiment d'effort ou de désir et la perception visuelle qui l'intéresse. Il s'établira ainsi une liaison immédiate entre le sentiment ou le désir et la perception du mouvement. Si nous voulions faire parler le bébé, comme William James son crabe, nous lui ferions dire: "Ce machin-là qui remue, et l'état de conscience extraordinaire qui vient de remplir tout l'univers, cela ne fait qu'un!"

Notre hypothèse de travail, c'est que tous les mouvements produits par le bébé, dans l'univers qui l'entoure, et tous les mouvements succédant simplement à un effort du bébé, vont être conçus de la même façon, jusqu'à ce que la résistance des choses et celle des personnes aient amené l'enfant à dissocier le monde en plusieurs centres actifs, et à construire parallèlement la notion de son moi et la notion d'êtres extérieurs considérés comme d'autres "moi."

Ces hypothèses peuvent paraître compliquées. Ce sont pourtant les plus simples possibles, et je crois qu'elles sont indispensables pour nous faire comprendre la genèse de l'imitation et la suite du développement de la causalité. En effet, si l'on prête au bébé une conscience innée de son moi, ou une localisation précoce de ses états de conscience (localisation du sentiment de l'effort musculaire, etc.) on ne comprend pas du tout, ni pourquoi il se met à imiter les autres, ni pourquoi il se livre à tant d'actes bizarres pour agir à distance sur le monde extérieur. Au contraire, dans notre hypothèse, le bébé est noyé dans un chaos d'impressions intéressantes, sans distinction entre les états internes et les choses extérieures, sans distinction entre un corps propre et des choses étrangères, et il se borne à associer des sensations non-localisées d'effort musculaire, etc., avec des mouvements perçus dans l'univers extérieur. Or, dès qu'il trouve moyen de conserver et de reproduire l'un quelconque de ces mouvements perçus, il s'essaye à en conserver et en reproduire le plus possible. Comme nous l'avons vu, la vie psychologique débute ainsi par la tendance à conserver et à reproduire les réalités vécues, de même que la vie organique consiste en une perpétuelle auto-conservation de l'organisme. Or, dans cette activité psychologique incessante, nous ne remarquons d'abord que ce qui réussit, c.-à-d. la reproduction des mouvements du corps propre, la "réaction circulaire" de Baldwin; mais cela n'est qu'un cas particulier. Passons maintenant aux autres, car, si notre hypothèse est exacte, c'est l'ensemble des mouvements extérieurs que l'enfant va s'essayer de conserver et de renouveler.

Le bébé commence par être le simple témoin, singulièrement intéressé, d'une multitude d'événements: une image qui passe dans le champ visuel, un son qui se fait entendre, un son accompagné d'une image, etc. Or, les efforts de l'enfant pour suivre des yeux et de la tête, puis pour suivre des mains, saisir, et surtout pour imiter dès qu'il le peut, montrent assez que le bébé n'est pas passif dans la contemplation de ce spectacle. Ici nous ne saurons jamais ce qu'il pense en percevant. Mais qui nous dit qu'il ne transpose pas, sur tout ce qu'il perçoit, l'ensemble de ses sentiments et en particulier des sensations d'effort et des sentiments de réussite analogues à ceux qui semblent caractériser ses premières réactions circulaires? Nous retrouverons l'hypothèse à propos de la genèse de l'imitation.

Ce qui est certain, c'est que, dès que le hasard produit la moindre convergence entre un mouvement du corps et un mouvement quelconque, le bébé utilise aussitôt le processus ainsi réalisé pour reproduire le mouvement extérieur qu'il a perçu. Voici un exemple:

A 5 mois et 8 jours, ma fille remue son corps entier et surtout ses pieds, sans les voir d'ailleurs; elle heurte par hasard une poupée suspendue au-dessus d'eux. Elle remarque alors le mouvement de la poupée, s'y intéresse, et reproduit, d'abord au petit bonheur, et ensuite, très vraisemblablement, par propre imitation, les mouvements de son corps entier et de ses jambes. Elle continue ainsi un instant. Je retire la poupée, puis, un moment après, je la remets. Elle renouvelle alors aussitôt ses mouvements, quoiqu'elle soit restée immobile entre temps. Je reprends alors la poupée durant une nouvelle pose, puis je la montre à nouveau, mais, au lieu de la remettre à sa place, je la suspends bien au-dessus, hors de portée des pieds. La petite se remet néanmoins à frétiller comme avant et regardant uniquement la poupée (et pas ses pieds). Il semble donc qu'elle concevait une liaison entre ses mouvements en général et celui de la poupée, et non une liaison précise entre le mouvement de ses pieds et celui de la poupée. Or ses mouvements à elle ne lui sont connus que par un ensemble de sensations musculaires et de sentiments de plaisir qu'elle ne localise sans doute pas, mais associe simplement à la vue de la poupée. C'est cette liaison vague, mais dont la mimique de contentement de la petite montre bien l'existence, que nous appelons la causalité par efficace.

On pourrait nous objecter que rien n'empêche le bébé de localiser ses sensations musculaires dans son pied et de prendre conscience du choc qui permet à ce pied de mettre en mouvement la poupée. Mais, outre le fait que la petite remue aussi bien ses pieds à une grande distance de la poupée qu'à côté d'elle, nous ne comprenons pas, si l'enfant a effectivement conscience des liaisons spatiales, les faits que nous allons décrire maintenant. Il est, en effet, remarquable que, dès que le bébé a découvert un de ces procédés d'action sur le monde extérieur, il l'applique à n'importe quoi, sans aucun souci des connexions spatiales. Il y a là une sorte d'assimilation motrice inconsciente qui permet au bébé de généraliser tous les procédés qu'il découvre.

Voici des exemples: I. A 7 mois et 16 jours, ma fille tire par hasard une poupée suspendue par un cordon au toit de son berceau, et constate les mouvements du toit ainsi produits. Elle recommence indéfiniment, par simple réaction circulaire. Or, un moment après, je lui présente ma montre, à une petite distance du cordon qui retient la poupée. Elle cherche à saisir la montre, se soulève, se contortionne, mais sans succès. Elle saisit alors de la main droite le cordon qui retient la poupée, tout en conservant la main gauche tendue vers la montre. Elle secoue alors violemment le cordon tout en désirant encore évidemment la montre, vers laquelle l'autre main est tendue.

II. 5 jours plus tard (à 7 mois 21 jours), je fais remuer, sans qu'elle me voie, le toit de son berceau. Dès que je m'arrête, elle saisit le cordon qui pend et tire à son tour. Notons ici que la petite n'a pas besoin de comprendre ce mécanisme pour l'utiliser. Il lui suffit, pour tirer le cordon, d'assimiler ce qu'elle voit à ce qu'elle a vu il y a 5 jours; or, c'est par hasard qu'elle a découvert, il y a 5 jours, les effets de cette traction. Un instant après, je fais passer un livre au-dessus de la tête de la petite, à la hauteur du toit du berceau, mais à une certaine distance de ce toit. Le livre va et vient ainsi un moment. Quand je m'arrête, la petite saisit aussitôt le cordon et le tire violemment un certain nombre de fois (à peu près une dizaine de fois). Je me garde alors de remuer le livre, mais un moment après je recommence. Dès que je m'arrête, la petite se remet à tirer le cordon, mais un moins grand nombre de fois. J'ai encore fait deux essais, et, à chacun, la petite a tiré le cordon, mais avec toujours moins de conviction.

Vous me direz sans doute que ces faits ne prouvent encore rien. Pour ma part, je les interprète comme des liaisons de causalité par efficace nées à l'occasion de liaisons phénoménistes. Le bébé exécute par hasard tel mouvement aboutissant à tel résultat, le tout accompagné de tel sentiment: par exemple, le fait de tirer un cordon produit une secousse dans le toit du berceau et le tout s'accompagne de sentiments d'effort et de réussite. Lorsqu'il désire voir se produire tel nouveau résultat, comme le balancement d'une montre ou d'un livre, son désir déclenche alors le geste de tirer le cordon comme il déclencherait n'importe quoi suivant le hasard des expériences précédentes, pourvu que le geste ainsi adopté ait été précédemment lié à un sentiment d'efficace. Mais il est clair que l'on pourrait voir là une conduite beaucoup plus rationnelle, une sorte d'induction élémentaire fondée sur la conscience des relations: c'est parce qu'il aurait vu la relation entre le cordon et le toit qu'il aurait tiré le cordon, et c'est parce qu'il aurait supposé des relations spatiales entre le cordon, la montre et le livre, qu'il aurait continué de tirer le cordon, etc. En réalité cette interprétation, qui paraît plus simple, pour le sens commun, est beaucoup plus compliquée que la nôtre, car elle suppose la formation, dans l'esprit de l'enfant, de groupes géométriques et physiques, et, en fait, ces groupes sont bien plus probablement le résultat, et non la cause, de l'expérimentation du bébé.

Quoiqu'il en soit, il convient, pour justifier notre manière de voir, de fournir deux contre-épreuves:

1° il nous faut montrer maintenant que de telles liaisons causales apparaissent entre des termes sans aucun lien spatial intelligible et 2° il nous faut montrer que là où il paraît n'y avoir que causalité par contact physique, le phénomène reste, en fait, le même.

Commençons par le premier point. Nous allons voir que les rapprochements les plus fortuits entre un geste du bébé et un mouvement extérieur, donne au bébé l'impression que son geste a de l'efficace. Voici des exemples:

I. A 7 mois et 30 jours, ma fille regarde le toit de son berceau, que je secoue sans qu'elle me voie. Quand je m'arrête, elle n'a pas l'idée de tirer le cordon qui en descend, mais elle remue doucement sa main contre la paroi interne du berceau. Elle fait cela dès que je m'arrête de secouer la toiture, et elle regarde attentivement cette toiture comme si le geste de la main allait déclencher les secousses. Elle continue un grand nombre de fois.

Le lendemain, je bats des mains devant elle. Dès que je m'arrête elle remue de nouveau sa main contre la paroi du berceau en regardant attentivement les miennes. Quand je reprends mon geste elle interrompt le sien et recommence quand je m'arrête.

Quelques heures après, je balance un chapeau à un mètre au-dessus de sa tête. Quand je m'arrête elle recommence son mouvement des mains, etc.

II. A 8 mois et 9 jours elle regarde une petite assiette que je balance devant elle. Les jours qui ont précédé, elle a pris l'habitude de manifester sa joie par le geste suivant: elle se cambre, en se dressant sur ses omoplates et sur ses talons et en se laissant ensuite retomber de tout son poids. Or, en voyant l'assiette, elle s'est cambrée de la même manière, par simple plaisir et évidemment sans aucune intention. Mais, au moment où elle s'est cambrée, j'ai par hasard fait repasser l'assiette. Alors, dès que je m'arrête, elle se cambre, et dès que je recommence elle s'arrête. Elle a donc sans doute le sentiment de produire le mouvement de l'assiette au moyen de son geste.

Quatre jours plus tard: elle regarde le toit de son berceau, que je fais osciller. Dès que je m'arrête, elle se cambre, sans doute, de nouveau, pour faire continuer le mouvement.

Les jours suivants, mêmes réactions mais appliquées à des situations toujours nouvelles. Elle se cambre ainsi devant des objets nouveaux, en manière d'expérience pour voir, devant des objets qu'elle cherche à saisir, devant moi pour me faire continuer les gestes que j'exécute, etc., etc.

Enfin, un mois après le début de cette série, elle se cambre encore en face d'un objet qu'elle désire voir s'ébranler.

On voit combien un rapprochement fortuit crée rapidement un sentiment de liaison causale, et cela sans aucun lien spatial intelligible[1].

III. A 11 mois, le fils du Dr R. de Saussure, est ébloui par une lampe que l'on allume en pleine nuit, en tirant un cordon, et cligne involontairement des yeux. Mais, sitôt remis, il cligne plusieurs fois des yeux devant le cordon, et cette fois intentionnellement, puis regarde si la lampe s'est allumée à nouveau. Le fait s'est reproduit un grand nombre de fois. Après 4 mois d'interruption, le bébé répète exactement la même conduite et en plein jour, alors que nous le menons devant le cordon.

On pourrait assurément interpréter ces gestes comme de simples mouvements de réponse, tels que l'enfant n'établisse lui-même aucune liaison de causalité entre le mouvement qu'il exécute et celui qu'il perçoit. Mais, si tel est sans doute le point de départ de ces conduites, elles prennent ensuite un sens nettement causal. D'une part, le geste n'est pas reproduit tant que le mouvement extérieur dure, mais dès qu'il cesse, et cela comme pour le faire recommencer. D'autre part, la mimique d'attente puis de plaisir de l'enfant ne laisse aucun doute sur le sentiment qu'il a d'être cause.

Ces exemples semblent donc bien indiquer que, pour le bébé, suivant la célèbre formule de Hume, "n'importe quoi peut produire n'importe quoi": qu'un geste soit associé par hasard à la perception d'un mouvement physique, et le geste devient cause du mouvement. Mais, il importe de le remarquer, ce phénoménisme ne reste jamais pur. Si le geste est conçu par le bébé comme la cause du mouvement, c'est que, grâce à son illusion solipsiste, l'enfant n'établit aucune différenciation entre le monde extérieur et son moi: ses sensations kinesthésiques et ses sentiments lui paraissent être en connexion directe avec les choses, et ainsi, à défaut de lien objectif entre le geste et le mouvement extérieur perçu en même temps, il s'établit sans doute dans la conscience du bébé un lien subjectif comme en témoigne la mimique d'attente puis de plaisir.

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[1] Pour la discussion de cet exemple, voir *Année psychol.*, vol. XXVI (1926), pp. 57–64.

Passons à notre seconde contre-épreuve et cherchons à montrer que même les liaisons causales qui paraissent les plus dûment spatiales et physiques sont au fond du même ordre. Prenons ici nos exemples dans la seconde année de l'enfant, car il sera d'autant plus probant de rencontrer plus tard ces mêmes liaisons à la place de la causalité simplement physique que l'on attendait.

I. A 1 an et 28 jours, ma fille apprend par imitation, à faire tomber, au moyen d'un bâton, un bouchon que je place sur le bord de son berceau. Elle arrive ainsi à attraper le bouchon, qui est situé trop loin pour qu'elle le saisisse directement avec la main. Deux jours après (à 1 an et 1 mois), elle a en mains une poupée, que je fais disparaître par-dessus le bord du berceau. Elle la cherche, essaye de la voir, et, comme elle ne réussit pas, elle s'empare du bâton et tape sur le bord du berceau, exactement à l'endroit où a disparu la poupée ! Il est bien difficile de ne pas voir dans ce geste un moyen pour faire revenir la poupée.

II. A 1 an, 6 mois et 8 jours, ma fille est assise dans un grand lit, en face d'un grand duvet qui fait montagne devant elle. Je place un jouet sur le sommet du duvet, et je tape sur le duvet jusqu'à ce que le jouet tombe peu à peu jusqu'à elle. Elle imite très bien ce geste. Un moment après, je mets le jouet sur une table, à un mètre de là, et séparée du lit par un grand espace vide. La petite se met néanmoins à taper sur le duvet, en regardant le jouet, comme s'il allait venir à cet appel !

La même réaction s'est renouvelée bien des fois.

III. Trois jours après, j'observe un fait du même ordre, mais entièrement spontané. La petite est dans une chambre. Elle ébranle une chaise dont le dossier touche une fenêtre ouverte; elle remarque alors que la fenêtre est déplacée et recommence à ébranler la chaise en regardant la fenêtre. Un moment après, elle heurte une autre chaise, à 1 m. 50 de là; elle la secoue alors et regarde aussitôt la fenêtre pour voir si celle-ci bouge de nouveau.

Bref, et on pourrait multiplier ces observations, même là où la causalité paraît à l'observateur entièrement spatiale et physique, il ne s'agit, pour l'enfant, que d'une liaison toute empirique et inintelligible. Du point de vue de la psychologie de réaction, il y a là pur phénoménisme. Du point de vue de la psychologie réflexive, il s'y ajoute sans doute un reste de causalité par efficace, bien qu'à l'âge d'un an et demi l'enfant ait évidemment acquis, grâce à l'imitation des autres et à la découverte de la résistance des choses, une notion plus précise de sa personnalité, et par conséquent des limites entre son moi et le monde extérieur.

Il resterait à parler des séquences causales extérieures à l'activité de l'enfant. Ainsi l'enfant constate que le choc de deux corps produit un son. Comment conçoit-il cette liaison? Mais nous n'avons malheureusement aucun moyen de nous renseigner. Tout ce que nous pouvons dire, c'est que étant donnée la conduite générale de l'enfant, ou bien de telles séquences ne lui apparaissent pas comme causales, ou bien il doit bien les concevoir sur le modèle de la causalité par efficace, du moins jusqu'au moment où il aura pris suffisamment conscience des résistances du monde extérieur pour établir une première distinction entre son moi et les choses.

Avant de conclure notre exposé, écartons encore une objection. On pourrait penser que si le bébé emploie n'importe quel procédé pour obtenir n'importe quel résultat, au gré des associations fournies par l'expérience, c'est simplement qu'il compte sur ses parents pour réaliser ses désirs. Dans cette hypothèse, le procédé employé par le bébé pour agir sur les choses constituerait simplement une sorte de langage à l'usage des personnes familières. Il n'y aurait pas magie, mais prière adressée à des dieux tout-puissants. Ainsi nous constatons que, vers 1 an ½ et 2 ans, l'enfant vient trouver ses parents dès qu'il a envie de quelque chose et leur dit simplement "S'il te plaît," sans prendre le soin de préciser ce qu'il veut, tant il est convaincu que ses parents connaissent tous ses désirs.

Mais si cette hypothèse devient vraisemblable quand l'enfant commence à parler, elle est insoutenable auparavant, et cela pour trois raisons.

La première raison est que vers 7–8 mois, à l'âge où le bébé commence à imiter autrui (et cet âge, chose intéressante, coïncide avec celui auquel apparaissent les faits de causalité par efficace que nous avons décrits), il n'établit évidemment aucune distinction précise entre sa personne et celle des autres. C'est même cette confusion qui, voyons nous, explique seule la genèse de l'imitation. Quelle que soit la manière dont on explique le mécanisme ou la technique de l'imitation, il reste en effet à savoir pourquoi l'enfant imite. Or les choses deviennent claires si l'on considère le besoin d'imitation comme un cas particulier de ce besoin si fondamental de l'activité psychique primitive, de faire durer tout ce qui a été une fois senti ou perçu. C'est ce besoin, avons nous vu, qui est au point de départ de la conquête progressive du monde par l'enfant. C'est ce besoin qui déclenche la réaction circulaire quand les mouvements à reproduire ont déjà été exécutés par le corps propre, et qui déclenche la causalité par efficace, lorsqu'il s'agit de mouvements radicalement étrangers au corps propre. Or, là où les mouvements perçus peuvent être directement assimilés à des mouvements déjà exécutés antérieurement par le corps propre (comme lorsque le bébé entend un son qu'il sait produire, ou voit un mouvement qu'il sait exécuter) le besoin de répétition se prolonge directement en imitation. A cet égard, l'imitation n'est au point de départ, qu'une confusion entre le moi et le monde extérieur, confusion provenant du fait que le bébé s'identifie avec tout ce qu'il perçoit. On ne peut donc en aucune manière voir dans l'imitation naissante une conduite sociale, quoique, dans la suite, l'imitation amène l'enfant à une prise de conscience de certains aspects de son moi. Il n'y a donc pas de conduites sociales au moment où commence à se développer la causalité, mais seulement des conduites solipsistes.

En second lieu, dès que l'imitation devient familière à l'enfant, elle est utilisée comme procédé d'action sur le monde extérieur, ce qui montre bien la parenté entre l'imitation et la causalité naissantes. Ainsi, à 8 et 9 mois, ma fille imitait souvent tel son ou tel geste jusqu'à ce qu'on l'ait répété, ou encore elle cherchait à se faire imiter elle-même. En outre, lorsqu'elle voulait obtenir tel résultat il lui arrivait d'employer indifféremment le procédé de l'imitation ou un des procédés décrits précédemment (remuer la main, se cambrer, etc.). Cela semble parler en faveur de l'hypothèse que nous cherchons à réfuter, que les procédés de la causalité par efficace sont en fait de simples moyens d'agir sur des personnes, moyens analogues à une sorte de langage. Seulement, cette causalité par imitation (sorte de magie imitative naissante) est aussi bien employé vis-à-vis des choses que vis-à-vis des personnes. Ainsi, à 9 mois et 9 jours, ma fille regarde un perroquet que je fais osciller devant elle (geste analogue à celui d'une salutation) sans qu'elle voie ni me sache présent. Dès que je m'arrête, la petite imite ce mouvement avec sa main, puis s'interrompt, puis recommence, cherchant évidemment à faire osciller le perroquet. Comme celui-ci ne bouge plus, elle finit par lui donner un coup tel pour le faire continuer. Il y a bien là la preuve que l'enfant ne distingue pas encore les personnes des choses.

Bien plus, et ceci est la troisième raison, ce qui montre le mieux que ces conduites primitives ne sont pas sociales c'est que, beaucoup plus tard, on trouve encore et à l'égard des personnes elles-mêmes des résidus manifestes de ces procédés quasi-magiques. Voici un bon exemple. A 1 an et 10 mois, ma fille tape avec une clef dans le fond d'un panier, et à chaque coup je pousse une sorte de cri qui la fait rire. Elle sait bien que c'est moi que crie, mais elle ne me voit pas. Je suis, en effet, caché derrière une paroi, quoique je l'observe moi-même. Après un moment, j'interromps mon cri. La petite, cherchant alors à le provoquer à nouveau, redouble ses coups, et, constatant l'échec, elle déplace légèrement le panier, le rajuste et recommence à taper. Je crie une fois, puis je m'arrête. Elle déplace alors de nouveau le panier, le rajuste encore une fois et recommence. Elle ne conçoit donc nullement mon cri comme une conduite intentionnelle, que je suis libre ou non d'exécuter. Ce cri n'est pas pour elle la réponse d'une personne à une autre personne. Elle l'assimile au contraire à une sorte de réaction physique nécessaire, qui dépend du choc des clefs contre le panier et de la position du panier. Or si de pareilles confusions se produisent encore à près de 2 ans, il est évidemment impossible de considérer les conduites de la première année comme sociales.

Il nous faudrait maintenant prolonger notre analyse et étudier les faits de phénoménisme accompagnés de causalité par efficace, qui se produisent jusque vers 4 et 5 ans. Nous verrions, en particulier, que cette croyance si répandue chez les petits, et suivant laquelle ils pensent avoir le pouvoir de faire avancer le soleil, la lune et les étoiles (pour ne pas parler des nuages et du ciel tout entier), a une structure psychologique analogue aux attitudes que nous avons étudiées. Mais il faut nous borner, d'autant plus que j'ai eu l'honneur de traiter, il y a quelques jours, dans une conférence donnée à la Cambridge Education Society, de la causalité chez l'enfant de 3 à 12 ans[1].

Cherchons donc à conclure cette rapide esquisse de la causalité chez le bébé. Deux résultats nous paraissent se dégager de ce qui précède. D'une part, ces faits de causalité semblent confirmer l'existence du dualisme que nous avons souligné dans la pensée de la première année: le dualisme de l'accommodation motrice et de l'assimilation autistique. D'autre part, les mêmes faits nous paraissent indiquer à nouveau que, pour la conscience même du bébé, accommodation motrice et assimilation autistique ne font qu'un, grâce au solipsisme qui identifie entièrement le moi du sujet au monde extérieur, autrement dit qui assure l'imbrication la plus étroite des états de conscience moteurs et affectifs dans l'ensemble des représentations visuelles, tactiles et auditives des choses elles-mêmes.

En ce qui concerne le premier point, il ne semble pas y avoir de difficulté. La causalité primitive est à la fois accommodation motrice et assimilation autistique. Elle est accommodation motrice dans la mesure où elle est phénoméniste. L'enfant fait un geste, il constate un résultat, il cherche à reproduire le même geste pour retrouver le même résultat: il n'y a dans ce cycle de mouvements que la prolongation de ce qui se passait lorsque le bébé apprenait à suivre des yeux, à saisir, à porter les objets à la bouche ou dans le champ visuel, etc. En ce sens on peut dire avec Hume que la causalité est née de l'habitude motrice. Mais la causalité primitive est aussi assimilation, au sens que nous avons donné à ce terme, et elle l'est dans la mesure où intervient le sentiment d'efficace. En effet, l'accommodation motrice ne va pas sans états de conscience concomitants (que ceux-ci apparaissent dès le début de l'évolution mentale ou en cours de route; qu'ils précèdent l'acte d'accommodation, ou lui succèdent, ou l'accompagnent, peu importe ici). Ce sont des désirs, des sentiments d'effort, d'attente, d'inquiétude, de satisfaction, de réussite, etc. Or il est clair, étant donnés ces états de conscience, que le bébé ne va pas établir une liaison toute objective entre ses gestes et leurs résultats, à la manière du savant qui note une simultanéité et se demande s'il y a relation causale. Les états affectifs vont au contraire être encastrés dans la perception du tout, et la conscience du geste efficace sera beaucoup plus, pour le bébé, la conscience des sentiments qui l'accompagnent que la connaissance des caractères objectifs de ce geste (trajectoire, contacts spatiaux, etc.). Lorsque l'enfant saura parler il exprimera cela en disant: "C'est moi qui produis tel résultat...," etc. Avant de parler, et, *a fortiori*, avant d'avoir la conscience de son moi, il sent quelque chose comme: "Ce désir, ou cet état de conscience d'effort, etc., va avec tel résultat...," etc. Or, s'il en est ainsi, la causalité est aussi bien assimilation autistique qu'accommodation motrice. En voyant un objet qui remue au loin, une montre qui se balance, etc., le bébé englobe aussitôt cette vision dans un schéma connu: il se cambre, par exemple, parce que le geste de se cambrer est le geste qui assure la réalisation des désirs. Autrement dit le désir de voir continuer ce qui intéresse en ce moment le bébé, déclenche le geste habituel de réalisation. Assurément, il y a dans cette forme de causalité autant d'accommodation que d'assimilation, puisque le bébé cherche (comme dans l'imitation elle-même, qui est une accommodation), à reproduire ce qu'il voit et non à le transformer en fonction de ses besoins; mais, dans la mesure où la causalité primitive contient de l'efficace, il y a assimilation, puisque les moyens que le bébé emploie pour agir sur les choses sont non pas des moyens adaptés à ces choses, mais les gestes qui constituent les simples concomitants habituels des désirs. L'évolution ultérieure de la causalité montre bien ce dualisme. Dans la mesure où l'élément d'assimilation sera éliminé de certains de ces processus, la causalité deviendra toute empirique ou phénoméniste, ce qui va de soi. Mais dans la mesure où l'élément d'accommodation sera éliminé de certains autres processus (sous l'influence de l'émotion, par exemple); la causalité deviendra magie pure: tel enfant inventera tel procédé absurde pour ne pas mourir, pour grandir, pour réaliser tel souhait, pour ne pas être puni, etc. Or cette causalité n'a plus rien d'adapté et est toute autistique.

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[1] Cette conférence paraîtra sous peu dans ce *Journal*.

Or, dans la genèse de la causalité, on ne peut pas dire, et c'est cela qui est important à noter, que l'élément d'accommodation précède l'élément d'assimilation, ou l'inverse. Dès les premiers pouvoirs que se découvre le bébé, il considère la chose sur laquelle il opère, comme soumise à son désir, c.-à-d. comme dépendant de ses états de conscience affectifs, ou même comme participant de ces états de conscience—ce qui est proprement de l'assimilation—et en même temps il exécute et répète aussi correctement que possible le geste qui vient d'être uni, par pur hasard, au résultat remarqué—ce qui est proprement de l'accommodation. Ainsi la causalité primitive nous présente une fusion intime de l'accommodation toute empirique de l'organisme aux choses et de l'assimilation systématique des choses aux états de conscience subjectifs, ce qui est conforme à toute l'activité en général de la première année.

Or, que ce paradoxe implique le solipsisme, c'est ce qui semble maintenant clair.. On ne comprendrait rien, en effet, à l'évolution ultérieure de la causalité, si le bébé ne confondait pas son moi avec le monde. Si la causalité primitive se ramenait, en effet, au pur phénoménisme, la causalité devrait ensuite se diriger dans le sens du pur empirisme. Or la causalité, de 3 à 8 ans, n'est en un sens qu'une longue projection du moi dans les choses: l'animisme et le dynamisme enfantin ne consistent qu'à prêter aux choses l'intelligence et l'effort musculaire. D'où cette attitude viendrait-elle, au moment où l'enfant conçoit le monde comme formé d'êtres et de personnes extérieurs à son moi et extérieurs les uns aux autres, si auparavant le bébé n'avait pas considéré les tableaux constituant son univers, comme le simple prolongement de ses désirs et de ses tâtonnements moteurs?

BRITISH NORMS FOR THE PRESSEY CROSS-OUT TEST

By MARY COLLINS.

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I. *The Test as used* (pp. 121–124).
II. *Results* (pp. 124–133).
  (*a*) *Affectivity* (pp. 124–127).
  (*b*) *The 'norms'* (pp. 127–131).
  (*c*) *Deviations* (pp. 131–133).
III. *Conclusion* (p. 133).

In a previous number of this *Journal*[1], I gave a preliminary report on character and temperament tests, and described among others the Pressey cross-out test. Since then, this test has been further studied, and the present paper gives a report of the results obtained.

1. The Test as used.

The form of the test used throughout the investigation was Form B, an 'expurgated' form specially devised for children, as distinct from Form A which is used exclusively for adults. In using the test in this country, the chief difficulty has been the number of American words encountered. In order to form a just estimate of the validity of this type of test, it was necessary to eliminate these words and substitute others. This was accordingly done. It may be noted that in test 3 no alteration of words was required and the test stands exactly as in its original form. In the other two tests, however, 19 words have been altered. In addition, two minor modifications have been introduced. The first concerns the changing of the order of the tests. If we call Pressey's order, 1, 2, 3, then the new order may be expressed as 3, 1, 2, *i.e.* the third test has been placed first, as it seems to me to be slightly more objective in character than the others as well as something of a 'shock absorber.' The second modification has to do with the encircling of the words in each line. Pressey postpones encircling until the crossing out has been completed in all three tests. I considered it better to complete the crossing out and encircling of each test before passing on to the next. Below are appended the tests in their modified form (Fig. 1).

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[1] Character and Temperament Tests, Oct. 1925.

Fig. 1.

Total t.........d.........dif.........

PRESSEY X-O TESTS

(FORM B. MODIFIED)

Write your Name, Age, Class, and School below.
Name...
Age last birthday ....
Class..............................
School ....

Directions:

READ CAREFULLY.

On the following pages there are three Tests. Each test occupies one page. The directions for each test are given at the top of the page. Work the tests in the order in which they come, finishing each test before you go on to the next. Be sure *to read the directions carefully.*

t.........d.........dif.........

TEST I.

Directions: Read over the twenty-five lists of words given below, and draw a line under *everything you like* or *are interested in.* You may underline as many or as few words as you wish. But be sure to underline everything that you like.

1. fortune-telling boating beaches mountains cinemas
2. camping tennis football eating soda-fountains
3. Beethoven Edison Napoleon Raphael Tennyson
4. kissing flirting pretty girls talkative girls athletic girls
5. studying dancing day-dreaming walking reading
6. talking elocution acrobats minstrels smoking
7. saxaphones bands flutes banjos singing
8. jazz ragtime hymns waltzes solos
9. good boys handsome boys rough boys rich boys leaders
10. fashions society clothes banquets games
11. palmistry pageants card-parties clubs socials
12. coffee onions mushrooms mustard ice-cream
13. sleeping teaching clerking loafing typewriting
14. actors musicians aviators engineers artists
15. Mowgli Tarzan D'Artagnan Hamlet Galahad
16. revivals deacons prayer priests church
17. French drawing English history science
18. babies sports sailors children animals
19. college business farming housekeeping travelling
20. business-men salesmen nurses teachers soldiers
21. newspapers poems books magazines debates
22. cards bargains switchbacks machinists electricity
23. chauffeurs doctors professors bankers grocers
24. hotels country cities crowds circuses
25. racing seaside fishing arguing joy-riding

Do not change any of the marks you have already made; leave them just as they are. But now go over each line carefully, and draw a circle round the *one* thing in each line that you like *best.* Be sure you have a circle round one word and only one word in each of the twenty-five lists. Do not miss out any list.

t.........d.........dif.........

TEST II.

Directions: Read over the twenty-five lists of words given below, and draw a line under *everything that you think is wrong*, everything that you think a person is blamed for. You may underline as many or as few words as you like, but be sure that you underline everything that you think is wrong.

1. begging smoking flirting spitting giggling
2. fear anger suspicion laziness contempt
3. dullness weakness ignorance meekness stinginess
4. fussiness recklessness silliness nagging fibbing
5. extravagance sportiness boasting deformity talking-back
6. clumsiness slang stubbornness delay hesitation
7. blarney debt bluff blues fretting
8. war revolution king socialism kidnapping
9. toughness meddling aristocrat boldness cad
10. worry thoughtlessness day-dreaming slowness spending
11. divorce gang overwork politics fault-finding
12. cowardice outcast swearing truancy temper
13. bashfulness stupidity cribbing queerness butting-in
14. disgrace immodesty fighting neglect leaving home
15. prize-fight snob betting grumbling conceit
16. idleness shabbiness pride chewing slyness
17. cunning absent-mindedness peddler cheapness sham
18. dispute greediness fox-trotting kidding dancing
19. over-dressing pitch and toss cheek stealing rivalry
20. strike lock-out union black-leg lawlessness
21. broker priest fanatic indecency bullying
22. fasting cards over-eating yelling gossip
23. tobacco speculating bribery drinking borrowing
24. dirtiness bragging sneering freak quarrel
25. teasing insanity cringing vomiting tale-telling

Do not change any of the marks you have already made; leave them just as they are. But now go over each line carefully, and draw a circle round the *one* thing in each line that you think is *worst*. Be sure you have a circle round one word and only one word in each of the twenty-five lists. Do not miss out any list.

TEST III.

t.........d.........dif.........

Directions: Read over the twenty-five lists of words given below, and draw a line under *everything about which you have ever worried or felt nervous or anxious*. You may underline as many or as few words as you like. But be sure you underline everything about which you have ever worried.

1. loneliness work forgetfulness school blues
2. sin headache fault-finding sneer depression
3. meanness clothes sickness looks unfairness
4. discouragement self-consciousness failure accidents worry

5. temper disease pain money awkwardness
6. fire nervousness germs insult disfigurement
7. noise manners habits medicine tuberculosis
8. God suspicions death religion weakness
9. moodiness teachers enemies lightning tiredness
10. boss homeliness rivals insanity police
11. dizziness wrecks storms falling longings
12. reciting popularity suffocating boys conscience
13. helplessness forgiveness poison business lessons
14. giggling friends crowds girls society
15. nightmares parties movies athletics morals
16. smoking clubs teasing dances jealousy
17. stylishness sleep food marriage stupidity
18. queerness soul whisperings drowning lies
19. ruin gun engagement fainting blushing
20. darkness burglars impulse politics dreams
21. neighbours family chums crying twitching
22. wit health dirt over-eating dogs
23. hysterics knives disposition persecution grave
24. stammering jokes cats books germs
25. detectives roughness flightiness employer childishness

Do not change any of the marks you have already made; leave them just as they are. But now go over each line carefully and draw a circle round the *one* thing in each line that you have worried about *most*. Be sure you have a circle round one word and only one word in each of the twenty-five lists. Do not miss out any list.

WHEN YOU HAVE FINISHED THE ENTIRE TEST—both the underlining and the circling—RAISE YOUR HAND.

2. Results.

This modified test was given to over 1500 children in Scotland and England, with ages ranging from 11 to 15. In collating the results, these ages have been kept strictly separate in order to allow any age differences to appear, and further the sexes have been separated in each age group. The norms obtained thus differ from the norms given by Pressey, for he gives general norms irrespective of age and sex, and norms which seem to have been obtained from 388 cases only of late adolescence (High School and College).

(*a*) *Affectivity.*

Let us consider in the first instance the scores in affectivity. The affectivity score is obtained by totalling the number of words underlined in each test.

Figure 2 shows the affectivity records for the girls only. Ages are shown along the horizontal axis, affectivity scores on the vertical axis.

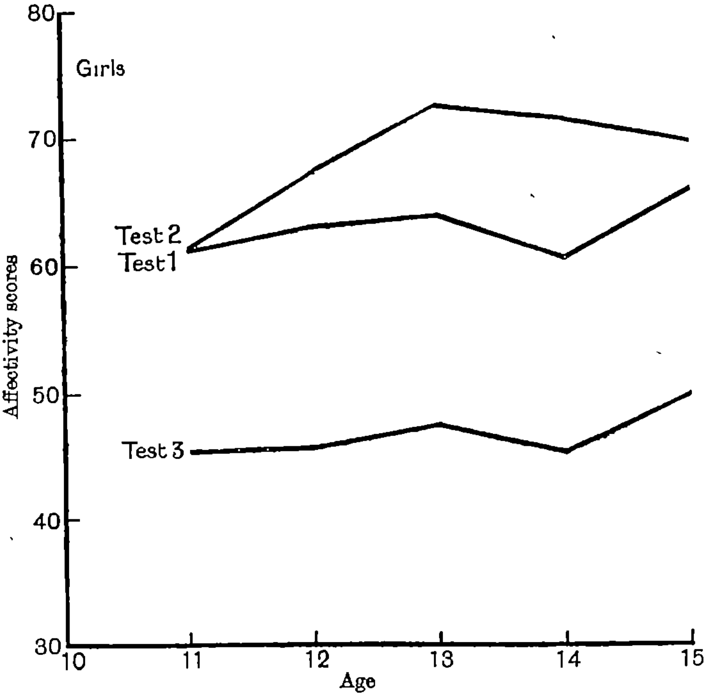
80
Girls
70
Test 2
Test 1
60
Affectivity scores
50
Test 3
40
30
10
11
12
13
14
15
Age

Fig. 2.

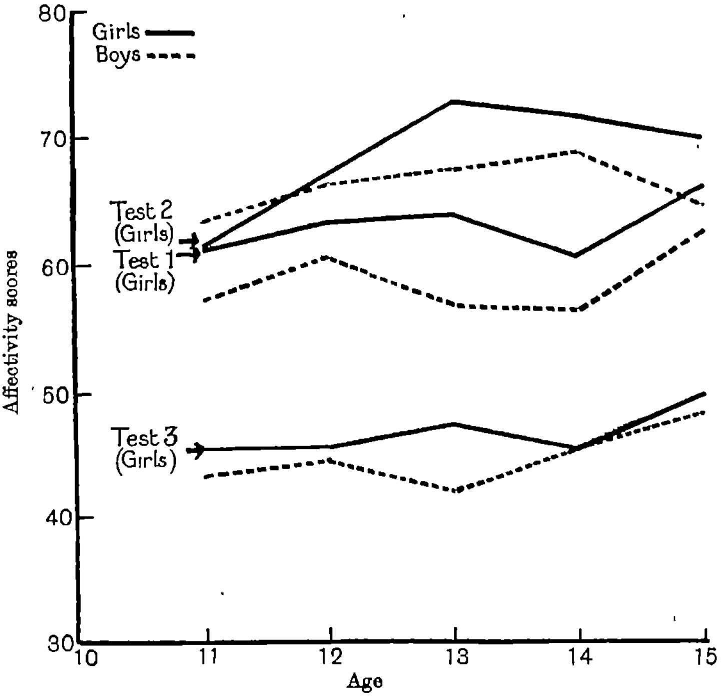
80
Girls
Boys
70
Test 2
(Girls)
Test 1
(Girls)
60
Affectivity scores
50
Test 3
(Girls)
40
30
10
11
12
13
14
15
Age

Fig. 3.

The three tests, it will be seen, vary as regards affectivity. Test 2 yields the highest affectivity, then Test 1, and the lowest is Test 3; that is, the test in which everything wrong is crossed out scores highest in affectivity, the test showing worries and anxieties scores lowest, the test which gives one's interests falling intermediately between these two. All three tests show a decided increase in affectivity at age 13, with a tendency to increase at age 12. The affectivity score rises in all three from ages 11 to 13, and is followed in all three by a decided decrease at age 14. Whether these results may be connected with the emotional development at adolescence is difficult to determine, but they certainly lay themselves open to such an explanation. The increase in Tests 1 and 3 at age 15 may be significant, but the cases are too few at this age to be reliable. In Fig. 3 are shown together the affectivity scores for both sexes, the heavy black line giving the girls' scores, the dotted line the boys'. The graphs are comparable, for the numbers in each age group are practically identical. The three tests show the same relative affectivity scores, and the tests arrange

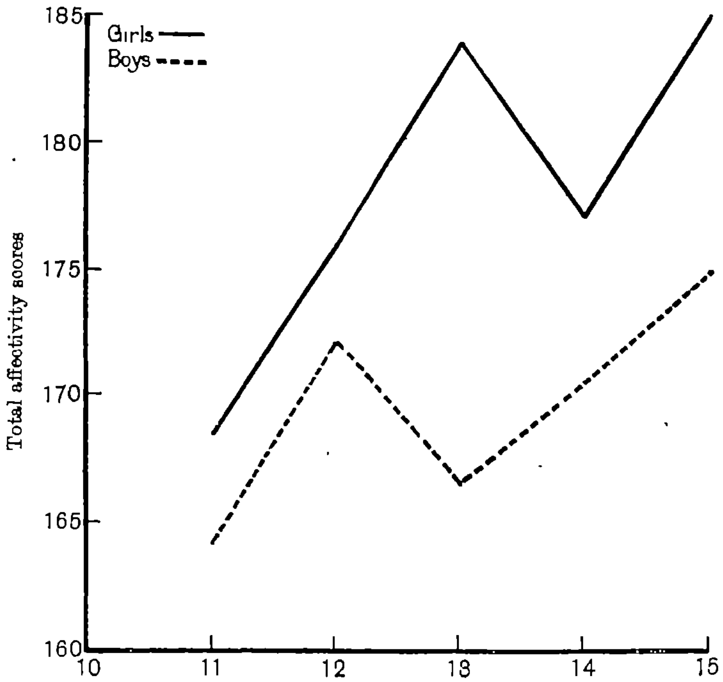

Fig. 4.

themselves in the order of affectivity 2, 1, 3 with the boys as with the girls; but in every case, for practically all ages, the score for boys is lower than that for girls, a decided sex difference. In Test 2 is shown the same increase from ages 11 to 13, perhaps a non-emotional stage before adolescence sets in. The increase at age 15 is again shown in two of the tests.

TABLE I. *Affectivity Scores.*

Boys	Test 1	Test 2	Test 3	Total
Age 11	57	63	43	163
,, 12	61	67	45	173
,, 13	57	67	42	166
,, 14	57	69	45	171
,, 15	62	65	48	175
Girls				
Age 11	61	62	46	169
,, 12	63	67	46	176
,, 13	64	72	47	183
,, 14	60	71	45	176
,, 15	66	69	50	185
Total				
Boys	58	67	44	169
Girls	63	69	46	178
Pressey	64	68	34	166

The total affectivity scores (Fig. 4), that is the total for all three tests, show these results even more clearly. This figure shows (1) the high affectivity score for girls as compared with boys; (2) the maximal increase at age 13 with girls with a decrease at age 14; the decrease at age 13 with boys then the gradual rise through 14 to 15. In actual figures the scores are given above (Table I).

(*b*) *The Norms.*

We come now to the words encircled in each line. After crossing out everything of interest in Test 1, the child is asked to go over the words again and to draw one circle in each line round the thing which is of most interest, in the second test the circle is drawn round the thing considered to be most wrong; in the third around the thing about which the child worries most.

It was surprising to find the British norms very different from the American ones. The test was given to the 1500 children principally to obtain the norms for those lines on which changes had been made. But as the results accumulated, it became clear that the norms for practically every line were going to be different. Further, by keeping the results of the boys apart from those of the girls, a sex difference appeared as shown by a distinctly different norm. Finally, within the sexes differences

FIG. 5. *Test I.*

		Girls				Boys			
	Pressey	11	12	13	14	11	12	13	14
1.	Boating	Boating	Boating	Cinemas or Boating	Boating	Boating	Boating	Boating.	Boating
2.	Camping	Tennis	Tennis	Camping or Tennis	Tennis	Camping or Football	Football	Football	Camping
3.	Edison	Tennyson	Tennyson	Tennyson	Tennyson	Tennyson or Napoleon	Napoleon	Napoleon	Napoleon
4.	Athletic girls	Pretty girls	Athletic girls	Athletic girls	Athletic girls	Pretty girls or Athletic girls	Athletic girls	Athletic girls	Athletic girls
5.	Dancing	Dancing	Dancing or Reading	Dancing or Reading	Dancing or Reading	Reading	Reading	Reading	Reading
6.	Talking	Minstrels	Minstrels	Acrobats	Elocution or Acrobats	Acrobats	Acrobats	Acrobats	Acrobats
7.	Singing	Singing	Singing	Singing	Singing	Bands or Singing	Bands	Bands	Bands
8.	Jazz	Hymns	Hymns	Solos or Hymns	Waltzes	Hymns	Hymns	Hymns	Jazz or Hymns
9.	Leaders	Good boys	Good boys	Good boys	Good boys	Good boys	Good boys	Good boys	Leaders or Good boys
10.	Games	Games	Games	Games	Games	Games	Games	Games	Games
11.	Socials	Socials	Socials	Socials	Socials	Socials	Socials	Socials	Socials
12.	Coffee	Ice-cream	Ice-cream	Ice-cream	Ice-cream	Ice-cream	Ice-cream	Ice-cream	Ice-cream
13.	Sleeping	Teaching	Teaching	Typewriting or Teaching	Typewriting	Typewriting	Typewriting	Typewriting	Sleeping
14.	Musicians	Actors or Artists	Actors or Musicians	Musicians	Musicians	Engineers	Engineers	Aviators or Engineers	Aviators
15.	Hamlet	Hamlet	Hamlet	Galahad or Hamlet	Hamlet	Tarzan	Tarzan	Tarzan	Tarzan
16.	Church	Church	Church	Church	Church	Church	Church	Church	Church
17.	Science	Drawing	Science	Drawing	Drawing or Science	Drawing or Science	Science	Science	Science
18.	Sports	Sports or Babies	Babies	Sports or Babies	Sports or Babies	Sports	Sports	Sports	Sports
19.	College	Housekeeping	Travelling or Housekeeping	Travelling	Travelling	Farming	Farming	Travelling or Farming	Travelling
20.	Business-men	Nurses or Teachers	Nurses	Nurses	Nurses	Soldiers	Soldiers	Soldiers	Soldiers
21.	Books	Poems or Books	Books	Books	Books	Books	Books	Books	Books
22.	Electricity	Switchbacks	Switchbacks	Switchbacks	Electricity or Switchbacks	Electricity or Switchbacks	Electricity	Electricity	Electricity
23.	Doctors	Doctors	Doctors	Doctors	Doctors	Doctors or Chauffeurs	Chauffeurs	Chauffeurs	Chauffeurs
24.	Country	Country	Country	Country	Country	Circuses	Circuses	Country	Country
25.	Racing	Seaside	Seaside	Seaside	Seaside	Fishing	Fishing	Fishing	Fishing

FIG. 6. *Test II.*

		Girls				Boys			
	Pressey	11	12	13	14	11	12	13	14
1.	Begging	Spitting	Spitting	Spitting	Spitting	Spitting	Spitting	Spitting	Spitting
2.	Laziness	Laziness	Laziness	Laziness	Laziness	Laziness	Laziness	Laziness	Laziness
3.	Stinginess	Ignorance	Stinginess or Ignorance	Stinginess	Stinginess	Stinginess or Ignorance	Stinginess or Ignorance	Stinginess	Stinginess
4.	Fibbing	Fibbing	Fibbing	Fibbing	Fibbing	Fibbing	Fibbing	Fibbing	Fibbing
5.	Boasting	Talking-back	Talking-back	Talking-back	Talking-back	Boasting or Talking-back	Talking-back	Talking-back	Talking-back
6.	Stubbornness	Slang	Slang	Slang	Slang	Slang	Slang	Slang	Slang
7.	Debt	Debt	Debt	Debt	Debt	Debt	Debt	Debt	Debt
8.	War	Kidnapping	Kidnapping	Kidnapping	Kidnapping	Kidnapping	Kidnapping	Kidnapping	War or Revolution
9.	Cad	Meddling or Cad	Cad	Cad	Cad	Cad	Cad	Cad	Cad
10.	Thoughtlessness	Spending	Spending	Spending	Thoughtlessness	Spending or Thoughtlessness	Spending	Spending	Spending or Thoughtlessness
11.	Divorce	Fault-finding	Divorce or Fault-finding	Divorce	Divorce	Fault-finding	Fault-finding	Divorce or Fault-finding	Divorce or Fault-finding
12.	Cowardice	Swearing	Swearing	Swearing	Swearing	Swearing	Swearing	Swearing	Swearing
13.	Cribbing	Stupidity	Stupidity or Butting-in	Cribbing or Butting-in	Cribbing or Butting-in	Butting-in	Butting-in	Cribbing or Butting-in	Cribbing
14.	Immodesty	Fighting or Disgrace	Neglect or Disgrace	Neglect	Neglect or Disgrace	Fighting	Neglect or Fighting	Neglect	Leaving home
15.	Snob	Betting	Betting	Betting	Betting	Betting	Betting	Betting	Betting
16.	Idleness	Slyness	Slyness	Slyness	Slyness	Slyness	Slyness	Slyness	Slyness
17.	Sham	Cunning	Cunning	Cunning	Cunning	Cunning	Cunning	Cunning	Cunning
18.	Greediness	Greediness	Greediness	Greediness	Greediness	Greediness	Greediness	Greediness	Greediness
19.	Swiping	Stealing	Stealing	Stealing	Stealing	Stealing	Stealing	Stealing	Stealing
20.	Lawlessness	Strike	Lawlessness	Lawlessness	Lawlessness	Lawlessness	Lawlessness	Lawlessness	Lawlessness
21.	Yellowness	Bullying	Bullying	Bullying	Bullying	Bullying	Bullying	Bullying	Bullying
22.	Gossip	Yelling	Gossip	Gossip	Gossip	Cards	Cards	Cards	Cards
23.	Bribery	Drinking	Drinking	Drinking	Drinking	Drinking	Drinking	Drinking	Drinking
24.	Dirtiness	Dirtiness	Dirtiness	Dirtiness	Dirtiness	Sneering	Bragging or Dirtiness	Dirtiness	Sneering
25.	Flunking	Tale-telling	Tale-telling	Tale-telling	Tale-telling	Tale-telling	Tale-telling	Tale-telling	Tale-telling

Fig. 7. *Test III.*

		Girls				Boys			
	Pressey	11	12	13	14	11	12	13	14
1.	School	Loneliness	Loneliness	School	School	Loneliness	Loneliness	School	School
2.	Sin	Sin	Sin	Sin	Sin	Sin	Sin	Sin	Sin
3.	Sickness	Sickness	Sickness	Sickness	Sickness	Sickness	Sickness	Sickness	Unfairness or Sickness
4.	Self-consciousness	Accidents	Accidents	Accidents	Failure	Accidents	Accidents	Accidents or Failure	Failure
5.	Money	Pain	Disease or Pain	Temper	Disease or Pain	Pain	Disease or Pain	Pain	Pain
6.	Nervousness	Fire	Fire	Fire	Fire	Fire	Fire	Fire	Insult
7.	Habits	Manners or Medicine	Manners or Medicine	Medicine	Medicine	Medicine	Medicine	Medicine	Manners or Medicine
8.	Weakness	Death	Death	Death	Death	Death	Death	Death	Death
9.	Enemies	Lightning or Enemies	Enemies	Enemies	Enemies	Enemies	Enemies	Enemies	Enemies
10.	Rivals	Police	Rivals or Police	Rivals	Rivals	Police	Police	Police	Police
11.	Longings	Storms	Storms	Wrecks or Storms	Wrecks or Storms	Storms	Storms	Storms	Storms
12.	Reciting	Suffocating	Suffocating	Suffocating	Reciting or Suffocating	Suffocating	Suffocating	Suffocating	Conscience or Suffocating
13.	Lessons	Poison	Poison	Lessons	Poison	Poison	Poison or Lessons	Lessons	Lessons
14.	Friends	Giggling or Crowds	Friends	Friends	Friends	Friends	Friends	Friends	Friends
15.	Morals	Nightmares	Nightmares	Nightmares	Nightmares	Nightmares	Nightmares	Nightmares	Nightmares
16.	Jealousy	Jealousy	Jealousy	Jealousy	Jealousy	Jealousy	Jealousy	Jealousy	Jealousy
17.	Stupidity	Stupidity	Stupidity	Stupidity	Stupidity	Stupidity	Stupidity	Stupidity	Sleep or Stupidity
18.	Lies	Drowning or Lies	Lies	Lies	Lies	Drowning or Lies	Lies	Lies	Lies
19.	Blushing	Fainting	Fainting	Fainting or Blushing	Blushing	Gun or Fainting	Fainting or Blushing	Blushing	Blushing
20.	Burglars	Burglars	Burglars	Burglars	Burglars	Burglars	Burglars	Burglars	Burglars
21.	Family	Crying	Crying	Chums	Chums	Crying	Chums	Chums	Chums
22.	Health	Dirt	Health	Health	Dirt	Dirt or Health	Dirt	Health	Health
23.	Disposition	Knives or Grave	Knives	Hysterics	Grave	Knives	Persecution or Knives	Knives	Disposition or Grave
24.	Germs	Germs	Germs	Germs	Germs	Germs	Germs	Germs	Germs
25.	Flightiness	Roughness	Roughness	Roughness	Roughness	Roughness	Roughness	Roughness	Roughness

appeared in the age groups. In marking a test, it will therefore be necessary, in order to obtain valid results, to score the individual according to the norm for the particular sex and age.

Fig. 5 shows the norms for the encircled words obtained from Test 1. The first column gives Pressey's norms, the next four the norms for girls of ages 11 to 14, the next four for boys of ages 11 to 14. Age 15 has been omitted because of the small number of the cases.

Attention may be drawn to the following points.

1. In several lines a distinct sex difference is shown. For example, in line 3 the norm is 'Tennyson' for girls, 'Napoleon' for boys except at age 11 where we have 'Tennyson' or 'Napoleon.' Similarly lines 5, 7, 14, 20, etc.

2. Boys of 11 often show a transition between the norms for girls and the norms for boys. For example, in line 3, the girls' norm is 'Tennyson,' the boys' 'Napoleon,' but for boys of age 11, it is 'Tennyson' or 'Napoleon.' The sex difference apparently shows itself most clearly at and after age 12.

3. A double norm also frequently appears where a change of norm takes place from one age to another in the same sex, for example, in line 13, with the girls.

4. Boys of 14 frequently break away from the usual norm. For example, line 2 'camping' ('football' for other ages), line 9 'leaders' or 'good boys' ('good boys' for other ages), etc.

The same points stand out clearly in the norms for Test 2 and Test 3. These are given in Figs. 6 and 7 respectively. Test 3, although absolutely unchanged from Pressey's original, shows different norms, and is interesting from this point of view. The norms are given in full, and without further comment, as they may prove useful to anyone using the test in this country.

(c) *Deviations.*

Finally we must consider the average deviations from the norms both as regards affectivity and idiosyncrasy.

Table II shows the average deviations from the norm as regards affectivity.

Table III shows the average deviation from the norms as regards idiosyncrasy, or the number of words encircled which differ from the norms established.

Table IV shows the deviations as regards idiosyncrasy obtained from giving the test to 100 delinquent boys of ages 11 to 14. All the boys belong to an industrial school. Most of them are delinquent cases sent from court, but a few have been sent because of undesirable environment at home. The average deviations of each age group have been calculated, and placed beside the normal average. A marked increase is noticeable with the delinquents in every case. The maximum deviation is of course 25. In Test 1 the delinquents of age 11 show a deviation of 14·6 (the range is from 11 to 19) as compared with 11. At age 12 we get 16·4 as

TABLE II. *Affectivity Scores.* (*Mean deviation.*)

Boys	Test 1	Test 2	Test 3	Total
Age 11	20	17	11·7	30·8
„ 12	14·2	16	12·8	33·5
„ 13	12·5	15·5	10·9	26·0
„ 14	9·6	14·8	13·6	34·2
„ 15	14·3	13·5	14·5	34·3
Girls				
Age 11	14·2	15·6	13·4	34·8
„ 12	11·9	15·5	11·4	29·8
„ 13	13·2	14·3	13·5	31·5
„ 14	11·2	13·2	18·1	27·1
„ 15	10·9	8·3	13·0	30·6

TABLE III. *Average Deviation from the Norm.*

	Girls				Boys			
Age	11	12	13	14	11	12	13	14
Test 1	11	12	11	10	11	12	11	11
Test 2	10	10	10	9	10	10	11	10
Test 3	11·5	13	14	13	12·5	13	14	13

General Average	Girls	Boys
Test 1	11	11·5
Test 2	9·5	10·5
Test 3	13	13·5

TABLE IV. *Average Deviation from Norm.*

Age		Normal	Delinquents
11	Test 1	11	14·6
	Test 2	10	12·7
	Test 3	12·5	13·2
12	Test 1	12	16·4
	Test 2	10	12·5
	Test 3	13	13·9
13	Test 1	11	15·4
	Test 2	11	11·1
	Test 3	14	14·4
14	Test 1	11	16·5
	Test 2	10	14·8
	Test 3	13	15·9

compared with 12 (range 11 to 22). At age 14 we have 16·5 compared with 11 (range 11 to 22). Increases are similarly shown with Tests 2 and 3. In Test 2 the ranges are very striking. For age 11 the range is from 6 to 17, for age 12 from 8 to 18, for age 13 from 7 to 24, and from age 14 from 9 to 21.

This difference between normals and delinquents seems to indicate that the Pressey Test may have some value as an instrument of diagnosis, and may aid the investigator in the attempt to determine the cause of the delinquency, but whether the results from it throw any light on the permanent emotional life of the individual seems to be negatived by repeating the test. The Pressey Test was given to a group of 100 boys and girls after the interval of a year, and then again to the same group after an interval of seven days. The results were disappointing. Very large variations were revealed. The longer interval was followed in some cases by an almost entirely different encircling, the deviations from the original being as large as 22 or 23, and no deviations being fewer than 10.

With the shorter interval of seven days this variability was somewhat lower, the average deviations being for the boys, 8·4, 9, 7, and 13·2 for the three tests respectively, and for the girls 9·5, 9·0, and 7·5. The averages, however, are too high, if the test is to have any validity. It means that a variation of $\frac{1}{3}$ is allowable.

3. CONCLUSION.

It seems fair to conclude, therefore, in accordance with these results, that the test, as a test of any aspect of 'character,' is not reliable. I still feel, however, that it may be helpful but that its value lies in the diagnostic field, as a guide to the emotional disturbance prevailing at the time at which the individual is examined[1].

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[1] I desire to acknowledge a grant from the Carnegie Trust for the Universities of Scotland for the purposes of this research.

(*Manuscript received* 9 *June*, 1927.)

AN INVESTIGATION INTO THE COMPARATIVE INTELLIGENCE AND ATTAINMENTS OF JEWISH AND NON-JEWISH SCHOOL CHILDREN

By MARY DAVIES and A. G. HUGHES.

I. *The Object of the Investigation* (pp. 134–135).
II. *The Investigation* (p. 135).
III. *The Results* (pp. 135–143).
  (*a*) *Differences in Marks* (pp. 140–141).
  (*b*) *Differences in Mental Ratios* (p. 141).
  (*c*) *Variations of Differences with Age* (p. 142).
  (*d*) *Sex Differences* (pp. 142–143).
IV. *Teachers' Estimates of Intelligence* (pp. 143–145).
V. *Conclusions* (pp. 145–146).

THE OBJECT OF THE INVESTIGATION.

This investigation was carried out to determine what differences (if any) are observable between Jewish and non-Jewish school children in intelligence and in some of the more important subjects of the elementary school curriculum. Three schools were chosen. The choice of schools was designed to rule out differences due to variations in school teaching and home training (except in so far as these latter are due to racial customs and traditions). The grounds of selection were mainly two:

1. That the schools should provide a fairly equal proportion of Jews and non-Jews in one and the same school.

2. That they should provide the best approximation to a similarity of social and economic status in Jewish as compared with non-Jewish families, *e.g.* one school contained poor Jews as well as poor non-Jews, while another school contained Jews and non-Jews from better-class homes. It was not possible, however, to find schools containing Jews and non-Jews of similar social status in exactly equal proportions, a point which will be discussed in a subsequent paper.

The characteristics of the three schools are as follows:

School A. 821 pupils (59 per cent. Jews, 41 per cent. non-Jews), situated in a good district in North London.

School B. 553 pupils (60 per cent. Jews, 40 per cent. non-Jews), situated in a moderately poor district in East London.

School C. 520 pupils (51 per cent. Jews, 49 per cent. non-Jews), situated in a very poor district in East London. It was believed that the Jewish children attending this school were as poor as could be found in any London school where the numbers of Jewish and non-Jewish children were approximately equal, and in the opinion of the head teacher, no poorer group of Jewish children could be found in any London school.

THE INVESTIGATION.

All the children from 8 to 14 years of age in the three schools were given the Northumberland Standardized Tests (1925 Series)[1]. These tests are printed booklets containing exercises in General Intelligence, English and Arithmetic. The General Intelligence booklet included exercises in reasoning, in finding opposites and similars, in detecting absurdities and in understanding instructions. English included silent reading, spelling, history, geography and a little composition. Arithmetic consisted of problems and of mechanical exercises in the four fundamental rules. Each child took all three tests on one day in the following order:

Morning. Arithmetic—recreation period—English.

Afternoon. General Intelligence.

The testing was carried out by the two investigators; and nearly two thousand children (in precise figures, 1894) were tested in this way[2].

THE RESULTS.

The results are given in Tables I–IX[3].

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[1] Prepared by Cyril Burt (University of London Press).

[2] We wish to acknowledge our indebtedness to the head teachers of the several departments for so kindly granting us facilities to carry out the tests, and both to them and to the assistant teachers for the trouble they have taken in collecting subsidiary information.

[3] Note on 'mental age' and 'mental ratio.'

The tests used were 'standardized' tests, that is to say, they had been given to large numbers of children and the average score at each age had been calculated. These average scores are given in column 2 in the tables, and are there called the 'Norm marks.' For example, in the Intelligence tests, the average score made by a large group of children who were 8 years old last birthday was 95 The average chronological age of this group must necessarily be 8½, since their ages range from 8·0 to 9·0. All children therefore who score 95 marks are said to have a mental age of 8½, no matter what their chronological age may be. Mental ages for attainments in English and Arithmetic are determined in a similar way.

A mental ratio is obtained by dividing a child's mental age by his chronological age. The ratio is usually expressed as a percentage.

Table I. *School A. General Intelligence.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	95	38	134	56·2	111	27	84	40·5	96	50 ± 8·1	15·0	1·25
9	136	45	166	53·7	109	30	139	62·5	101	27 ± 9 4	8·0	0 75
10	174	46	195	56·0	106	37	169	55 5	99	26 ± 8 3	7·0	0·75
11	205	37	216	32·6	104	24	200	50·1	98	16 ± 7·8	6·0	0 70
12	232	36	223	45·3	97	23	199	55·1	90	24 ± 8·2	7·0	0·85
13	253	28	256	36·9	101	24	219	59 4	89	37 ± 8 2	12·0	1·60
14	267	7	255	—	94	4	251	—	92	4 ±	2·0	0·30
Girls.												
8	95	37	116	44·7	105	26	114	51·3	105	2 ± 8·4	0·0	0·00
9	136	52	175	54·8	111	28	131	59·7	98	44 ± 9·2	13·0	1·25
10	174	36	192	52 0	105	30	165	57 9	98	27 ± 9·2	7·0	0 75
11	205	50	224	40·9	105	25	209	42·7	101	15 ± 6·8	4·0	0·50
12	232	39	234	32·5	101	26	222	58·5	97	12 ± 8·5	4·0	0·50
13	253	33	226	49·4	90	28	228	39·4	91	−2 ± 7·7	−1 0	−0·15
14	267	4	230	—	85	1	235	—	87	−5 ±	−2·0	−0·30

Table II. *School A. English.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	101	39	161	58·5	116	28	109	53·3	103	52 ± 9·2	13·0	1·10
9	149	45	204	60 5	119	30	159	67 7	103	45 ±10·3	16·0	1·50
10	183	45	226	63·7	114	37	199	59·8	105	27 ± 9 2	9·0	0·95
11	213	36	252	35·0	115	24	235	56·2	107	17 ± 8·7	8·0	0·90
12	240	36	256	59·2	107	23	231	88·6	97	25 ±14·2	10·0	1·25
13	259	28	292	53 0	114	24	244	80·3	94	48 ±12·9	20·0	2·70
14	279	7	276	—	99	4	267	—	95	9 ±	4·0	0·60
Girls.												
8	101	37	147	44·3	111	26	131	54·7	107	16 ± 8·8	4·0	0·35
9	149	54	196	41·6	115	30	162	58·1	103	34 ± 8 1	12·0	1·15
10	183	36	213	64·8	110	30	184	58 9	100	29 ±10·2	10 0	1·05
11	213	50	249	55·2	113	25	237	56 6	108	12 ± 9·3	5·0	0·55
12	240	39	264	57·1	110	26	249	67 2	103	15 ±10·8	7 0	0·85
13	259	33	255	65·1	98	28	250	54·4	96	5 ±10·1	2·0	0·25
14	279	4	268	—	96	1	234	—	85	34 ±	11·0	1·60

TABLE III. *School A. Arithmetic.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	76	39	91	38 7	104	27	61	31·9	95	30 ± 5·8	9·0	0·75
9	114	45	136	46·4	107	30	112	45·3	99	24 ± 7·3	8·0	0·75
10	150	46	168	47·6	105	37	138	45·4	96	30 ± 6·9	9 0	0·95
11	182	35	194	32·3	104	24	157	44·0	93	37 ± 7·1	11 0	1·25
12	206	36	189	70·3	94	23	168	55 0	88	21 ± 11·1	6·0	0·75
13	227	28	236	40·9	105	24	178	58·1	84	58 ± 9·6	21·0	2·85
14	241	7	205	—	86	4	185	—	79	20 ±	7·0	1·00

Girls.

8	76	37	92	32 4	105	26	70	36·4	98	22 ± 6 0	7·0	0·60
9	114	54	138	40·1	107	30	105	43·8	97	33 ± 6·5	10·0	0·95
10	150	36	157	58·6	102	30	133	50 7	95	24 ± 9·0	7·0	0·75
11	182	50	184	37 5	101	25	171	48·3	97	13 ± 7 4	4·0	0·45
12	206	39	201	51·1	98	26	177	56·1	90	24 ± 9·2	8·0	1·00
13	227	33	196	52·8	89	28	191	53·1	87	5 ± 9·2	2·0	0 25
14	241	4	204	—	85	1	160	—	74	44 ±	11·0	1·60

TABLE IV. *School B. General Intelligence.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	95	23	143	46·4	115	9	120	30·3	106	23 ±	9·0	0·75
9	136	25	163	46·2	108	16	115	44·6	93	48 ± 9·7	15·0	1 40
10	174	38	195	64·9	106	21	157	68 2	95	38 ± 12·3	11·0	1·15
11	205	25	230	49·1	108	27	175	65·3	91	55 ± 10·8	17·0	1·95
12	232	21	241	32·2	103	13	221	69·2	97	20 ± 13·8	6·0	0·75
13	253	17	252	27·0	99	24	211	55·7	86	41 ± 8·8	13 0	1·75
14	267	6	245	—	90	7	194	—	76	51 ±	14 0	2·05

Girls.

8	95	26	108	53·6	103	12	66	29·8	93	42 ± 9·2	10·0	0·85
9	136	29	159	56·6	107	18	129	49·3	98	30 ± 10·5	9·0	0·85
10	174	36	186	70·8	102	21	162	47 0	97	24 ± 11·1	5·0	0 55
11	205	33	228	53·9	108	30	189	59·6	95	39 ± 9·7	13·0	1·50
12	232	17	216	45·2	95	16	199	50·7	90	17 ± 11·7	5·0	0·60
13	253	19	233	57·1	93	18	232	38·1	93	1 ± 10·6	0·0	0·00
14	267	5	255	—	94	1	266	—	100	−11 ±	−6·0	−0·85

TABLE V. *School B. English.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	101	7	173	—	120	2	194	—	130	-21 ±	-10·0	-0·85
9	149	22	192	46·4	114	9	178	—	109	14 ±	5·0	0·45
10	183	37	238	70·9	119	17	187	56·5	101	51 ±12 1	18·0	1·90
11	213	25	263	54·2	120	22	205	51 2	97	58 ±10 3	23·0	2 65
12	240	21	272	49·7	114	13	242	69·8	101	30 ±14·9	13 0	1·60
13	259	17	293	46·2	114	24	245	62 8	94	48 ±11 9	20·0	2·70
14	279	6	288	—	104	7	241	—	87	47 ±	17·0	2·45

Girls.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	101	15	128	55 1	106	9	95	28·1	99	33 ±	7·0	0·60
9	149	24	197	64·2	116	12	159	59·9	103	38 ±14·6	13 0	1·25
10	183	32	227	70·4	115	16	204	47 0	107	23 ±11·5	8·0	0 85
11	213	33	259	63·4	118	29	213	56 6	100	46 ±10 3	18·0	2 05
12	240	17	239	52·1	99	15	233	50·4	98	6 ±12·2	1 0	0·10
13	259	20	262	74·2	102	18	272	55 0	105	-10 ±14 2	- 3·0	-0·40
14	279	5	279	—	100	1	309	—	114	-30 ±	-14·0	-2·05

TABLE VI. *School B. Arithmetic.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm. mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	76	23	93	33·7	105	9	70	—	98	23 ±	7·0	0·60
9	114	25	137	49 7	107	16	94	37·0	93	43 ± 8·2	14·0	1·35
10	150	38	181	71·4	110	21	134	50·0	95	47 ±10·7	15 0	1·55
11	182	25	201	44·3	107	27	155	54 9	92	46 ± 9·3	15·0	1·70
12	206	21	207	78 4	100	13	185	54·3	93	22 ±15 4	7 0	0·85
13	227	17	220	51·0	97	24	181	48·8	85	39 ±10·7	12 0	1·60
14	241	6	209	—	87	7	167	—	76	42 ±	11 0	1·60

Girls.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm. mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	76	26	97	34·3	106	12	69	26·0	98	28 ± 6 8	8 0	0·70
9	114	30	128	48·3	104	17	116	50·9	101	12 ±10 3	3 0	0·30
10	150	35	163	64 6	104	21	141	34 4	97	22 ± 8 9	7 0	0·75
11	182	33	202	51·4	108	31	157	43 1	93	45 ± 7 6	15 0	1·70
12	206	17	192	44·8	95	16	180	37·9	91	12 ± 9·7	4·0	0 50
13	227	20	203	45 9	91	18	190	37·0	87	13 ± 9·0	4·0	0·55
14	241	5	230	—	95	1	229	—	94	1 ±	1·0	0·15

TABLE VII. *School C. General Intelligence.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	95	24	99	51·8	101	9	52	39·1	89	47 ±	12·0	1·00
9	136	20	125	53·6	96	25	70	40 3	83	55 ± 9·7	13·0	1·25
10	174	33	159	59·5	96	21	127	62·8	87	32 ± 11·6	9 0	0 95
11	205	21	190	55·4	95	24	150	63·0	85	40 ± 11·9	10 0	1·15
12	232	24	202	53·2	91	19	175	74·0	83	27 ± 13·6	8·0	1·00
13	253	24	221	44·1	89	18	166	58·1	75	55 ± 11·1	14·0	1 90
14	267	2	232	—	86	2	215	—	81	17 ±	5 0	0·70

Girls.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	95	23	104	56·3	102	17	56	49·1	90	48 ± 11·1	12·0	1·00
9	136	20	115	52·2	93	22	74	51·1	84	41 ± 10·8	9·0	0·85
10	174	17	154	46·4	95	17	117	62·4	85	37 ± 12·2	10 0	1·05
11	205	25	185	60·8	94	22	142	64·3	83	43 ± 12·3	11 0	1·25
12	232	11	172	64·4	83	26	166	45·1	82	6 ± 14·4	1·0	0 10
13	253	18	228	16·0	91	26	163	54·9	75	65 ± 8·0	16·0	2·15
14	267	5	210	—	80	5	182	—	73	28 ±	7·0	1·00

TABLE VIII. *School C. English.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	101	25	118	60·2	104	9	67	—	94	51 ±	10 0	0 85
9	149	20	158	59·7	102	25	88	50·1	87	70 ± 11·3	15·0	1·40
10	183	32	197	71 4	105	21	143	69·1	88	54 ± 13·2	17·0	1·80
11	213	21	217	63 6	101	24	183	69·8	91	34 ± 13·4	10·0	1·15
12	240	24	233	57 4	97	19	204	86·0	89	29 ± 14·4	8 0	1 00
13	259	24	269	48·6	104	18	202	61·1	82	67 ± 11·8	22 0	2·95
14	279	2	278	—	100	2	249	—	89	29 ±	11·0	1·60

Girls.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	101	24	121	52 3	105	16	67	50·2	94	54 ± 11·1	11·0	0·95
9	149	20	140	56 3	98	22	86	60 6	87	54 ± 12·1	11·0	1 05
10	183	19	171	50 4	96	18	144	55 2	89	27 ± 12·1	7·0	0·75
11	213	25	208	73·1	98	22	161	75·9	85	47 ± 14·7	13 0	1·50
12	240	11	218	87·1	93	26	187	53·4	84	31 ± 19·0	9 0	1·10
13	259	18	250	30·7	96	26	179	60·2	76	71 ± 9·4	20·0	2·70
14	279	5	238	—	86	5	195	—	74	43 ±	12·0	1·75

TABLE IX. *School C. Arithmetic.*

Boys.

		Jews				Non-Jews				Differences Jews—Non-Jews		
Age last Birthday	Norm mark	No. in group	Average mark	Standard deviation	Average mental ratio	No. in group	Average mark	Standard deviation	Average mental ratio	In average marks	In average mental ratios	In average mental ages
8	76	25	85	35 4	102	9	62	—	96	23 ±	6·0	0·50
9	114	20	133	54·8	106	25	77	45·6	89	56 ± 10 3	17·0	1 60
10	150	32	149	67 6	100	21	119	49 5	91	30 ± 10·9	9·0	0 95
11	182	21	203	42·2	108	24	139	63·5	88	64 ± 10 7	20·0	2 30
12	206	24	183	34·1	92	19	147	52 9	83	36 ± 9·3	9·0	1·10
13	227	24	199	75·3	90	18	160	35 6	79	39 ± 11·8	11·0	1·50
14	241	2	211	—	87	2	186	—	80	25 ±	7 0	1·00

Girls.

8	76	23	75	36 7	99	16	51	26·7	93	24 ± 6·8	6·0	0·50
9	114	20	80	35 9	90	22	69	33 2	87	11 ± 7·2	3·0	0·30
10	150	19	125	48 8	93	18	97	52·9	85	28 ± 11·3	8·0	0 85
11	182	25	155	56·9	92	22	113	54 8	81	42 ± 12 0	11 0	1·25
12	206	11	161	66·5	86	26	144	40·3	82	17 ± 14·5	4 0	0·50
13	227	18	197	43 1	89	26	142	33·8	75	55 ± 8·4	14·0	1·00
14	241	5	161	—	74	5	145	—	71	16 ±	3·0	0·45

(*a*) *Differences in Marks.*

The most striking feature is the general superiority of the Jewish children, whether boys or girls, alike in Intelligence and in attainments in English and Arithmetic, at almost every age from 8 to 13, and in every type of school. In only two groups (girls of 13 in School A, as regards Intelligence, and girls of 13 in School B, as regards English) are the non-Jewish children superior, and in either case this superiority is all but negligible. In the following groups the Jewish superiority is so small as to be by itself statistically insignificant.

School A.	Girls of 8.	Intelligence.
	Girls of 13.	English and Arithmetic.
School B.	Girls of 13.	Intelligence.
	Girls of 12.	English.
School C.	Girls of 12.	Intelligence.

Omitting twenty-six age groups where the numbers of children are very small (less than ten), we have appreciable differences in favour of the Jewish pupils in ninety-three groups out of one hundred.

Statistical Significance of the Differences. From the statistical standpoint it must be noted that the great majority of the age groups from which the differences have been calculated contain groups of Jews and non-Jews numbering less than thirty in each. Too great importance must therefore not be attached to the actual numerical differences shown in the tables. If we take as our criterion of significance the usual requirement that the difference must be at least three times its probable error, we find that 52 per cent. of the differences are significant. Taking into account the comparatively uniform differences shown in all the groups, it seems fair to suggest that the non-significance of many of the remaining differences is due to the small numbers inevitable in age groups in this investigation.

(*b*) *Differences in Mental Ratios.*

In order to make the results more readily intelligible, the average marks for each age group have been converted into mental ratios, and the 'racial' differences expressed in these terms, and in terms of mental ages, are given in the last two columns of Tables I to IX. If, neglecting as before the twenty-six groups where the numbers are very small, we combine the age groups and calculate the average mental ratios, we find the following differences:

TABLE X. *Differences in average mental ratios.* (*Jews and non-Jews.*)

(In every case the figures indicate superiority of Jewish children.)

	General intelligence			English			Arithmetic		
School	Boys	Girls	Boys and girls	Boys	Girls	Boys and girls	Boys	Girls	Boys and girls
A	9·4	5·6	7·5	12·5	7·6	9·9	10·2	7·0	8·6
B	15·4	8·2	11·6	19·1	8·8	13·5	14·1	8·4	11·1
C	11·8	11·6	11·7	14·8	13·3	14·4	13·0	9·2	11·6
A+B+C	11·8	9·2	10·5	14·8	11·1	13·0	12·1	8·9	10·6

On an average the Jewish boys are distinctly in advance of the non-Jewish boys attending the same school; at age 10 the difference is equivalent to over a year in Intelligence and in Arithmetic, and to a year and a half in English. The superiority of the Jewish girls over the non-Jewish girls is not quite so marked; at age 10 it is equivalent to slightly less than a year (0·9) in Intelligence and in Arithmetic, and to a little more than a year (1·1) in English. The differences between the boys are relatively constant in the three schools; but the differences in Intelligence and English between the girls are considerably less in the good district than they are in the poorest district.

(*c*) *Variations of Differences with Age.*

The following table gives the average differences in mental ratios at each age, combining the results of the three schools:

Table XI. *Average differences in mental ratios at each age.*
(*Jews and non-Jews: three schools.*)

	General intelligence			English			Arithmetic		
Age	Boys	Girls	Boys and girls	Boys	Girls	Boys and girls	Boys	Girls	Boys and girls
8	12·0	7·7	9·8	11·5	7·3	9·0	7·3	7·0	7·1
9	12·3	10·3	11·3	12·0	12·0	12·0	13·0	5·3	9·1
10	9·3	7·7	8·5	14·7	8·3	11·5	11·0	7·3	9·1
11	11·0	9·3	10·1	13·7	12·0	12·8	15·3	10·0	12·6
12	7·0	3·3	5·1	10·3	5·7	8·0	7·3	5·3	6·3
13	13·0	5·3	9·1	20·7	6·3	13·5	14·7	6·7	10·7
14	(7·0)	(−0·3)	(3·3)	(10·7)	(3·0)	(6·8)	(8·3)	(5·0)	(6·7)

The total number of children (Jews and non-Jews) in the 14-year-old age groups was only 46.

The Jewish children maintain a more or less uniform superiority until the age of 12 when there is apparently a decided drop in the differences in Intelligence, English and Arithmetic. But this drop can hardly be attributed to a precocious arrest of development, for the Jewish superiority is more than regained by the boys of 13 and a similar tendency (though not so marked) is shown by the girls. When we consider that the fall in the differences at age 12 is certainly due to some extent to the fact that about the age of 11, more Jewish than non-Jewish children have been transferred to secondary schools, either with scholarships or as fee-paying students, this rise in Jewish superiority is the more remarkable. It may be due to the fact that pubertal influences occur at different ages; for example, the Jewish boy may be affected about the age of 12, and the non-Jewish boy somewhat later.

(*d*) *Sex Differences.*

If now we find the differences in average mental ratios between boys and girls (keeping the race constant) we get the following results:

Table XII. *Differences in average mental ratios between boys and girls.*

	Intelligence	English	Arithmetic
Jews	+1·4	+3·7	+3·3
Non-Jews	−1·2	0·0	+0·1

A plus sign indicates that boys are superior to girls. A minus sign indicates that girls are superior to boys.

In intelligence the Jewish boys are slightly superior to the Jewish girls: this result is reversed among non-Jewish children. In attainments there is no difference between boys and girls among non-Jewish children, but among Jewish children the boys' superiority is greater than it was in intelligence. On an average, the difference is equivalent to about four months' progress about the middle of the school career.

TEACHERS' ESTIMATES OF INTELLIGENCE.

As a tentative check upon the results of the intelligence tests, the teachers were asked to rate their pupils for intelligence according to the Biometric Laboratory Scale[1]. In cases of doubt the class teachers conferred with other teachers who knew the pupils, and with the head teacher. The estimates were of course made in complete ignorance of the test results.

The percentage number of children in each category is shown in Table XIII. For purposes of comparison the results obtained by Pearson and Moul[2], who used the same method, are appended. Their results refer to children of the Jews' Free School, Whitechapel, and were obtained in 1913.

In all three schools the Jewish children are, according to their teachers' estimates, distinctly superior to the non-Jewish children in the same school. These results confirm those obtained by the tests.

If we compare one school with another, difficulties arise. For example, according to the teachers' estimates the non-Jewish boys of the poorest school are equal in intelligence to those of the best school: the Jewish girls of the poorest school are definitely superior to those of the best school. Thus it would appear that though the teachers' estimates in each school may be reliable in so far as they place the children in a relative order of intelligence, no absolute standard common to all schools is obtained by giving teachers verbal descriptions of each intelligence category[1]. This being so, no trustworthy conclusions can be drawn by comparing the results of the Pearson-Moul investigation with those of the present one as given in Table XIII. The matter may be simplified by combining the categories so as to make a four-fold division. This has been done as suggested by Pearson and Moul[2]; the results are given in Table XIV, together with the results of other investigations which Pearson and Moul used for comparison with their own data.

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[1] The scale is as follows. (In the original scale the letters are reversed, F being the highest, and A the lowest grade. The lettering was changed in order to fit in with the customary usage of these letters in school grading):

A. "*Very able.* Quite exceptionally able intellectually, as evidenced by the school record."
B. "*Distinctly capable.* A mind quick in perception and in reasoning rightly about the perceived."
C. "*Fairly intelligent.* Ready to grasp and capable of perceiving facts in most fields; capable of understanding without much effort."
D. "*Slow intelligent.* Slow generally, although possibly more rapid in certain fields; quite sure of knowledge when once acquired."
E. "*Slow.* Very slow in thought generally, but with time understanding is reached."
F. "*Slow dull.* Capable of perceiving relationship between facts in some few fields with long and continuous effort; but not generally nor without much assistance."
G. "*Mentally defective.* Capable of holding in the mind only the simplest facts, and incapable of perceiving or reasoning about the relationship between facts."

[2] Karl Pearson and Margaret Moul, "The Problem of Alien Immigration into Great Britain, illustrated by an examination of Russian and Polish Jewish children," *Annals of Eugenics*, 1925, I, 125.

TABLE XIII. *Percentage Distributions of Intelligence.*
(*Teachers' Estimates—Biometric Laboratory Scale.*)

No. of children		Very able	Capable	Intelligent	Slow	Dull	Very dull	Mentally defective
School A:								
166	Non-Jewish boys	0·6	7·2	34·3	35·0	14·5	7·2	1·2
164	Non-Jewish girls	1·8	7·3	25·0	31·1	18·9	15·9	0·0
232	Jewish boys	0·4	21·1	42·7	25·4	6·5	3·0	0·9
251	Jewish girls	3·2	10·7	32·3	23·5	21·1	9·2	0·0
School B:								
115	Non-Jewish boys	0·0	13·9	36·5	23·5	15·7	9·6	0·9
114	Non-Jewish girls	0·0	7·0	26·3	24·6	24·6	16·7	0·9
155	Jewish boys	14·8	30·3	35·5	11·6	4·5	3·2	0·0
171	Jewish girls	4·1	22·8	31·6	20·5	14·0	7·0	0·0
School C:								
115	Non-Jewish boys	0·0	5·2	38·3	33·0	15·7	7·8	0·0
134	Non-Jewish girls	0·0	3·7	20·1	27·6	28·4	20·1	0·0
148	Jewish boys	0·0	16·9	49·3	26·4	6·7	0·7	0·0
122	Jewish girls	1·6	13·9	48·4	25·4	8·2	2·5	0·0
Pearson-Moul investigation:								
619	Jewish boys	3·1	12·9	36·2	34·7	11·0	1·9	0·2
585	Jewish girls	1·2	6·3	25·5	37·8	18·1	10·4	0·7

In no other school do we find Jewish children as poor in intelligence as Pearson and Moul found in the particular school studied by them. The most striking divergence in the results is perhaps to be seen in the case of the girls; Pearson and Moul found that "Jewish girls have less intelligence than Gentile girls in any type of Council school," whereas our data, obtained by the same methods, seem to indicate that the Jewish girls are at least equal in intelligence to the non-Jewish girls as measured in the London Pioneer Survey, and are distinctly superior to the non-Jewish girls as measured in this investigation. When making these comparisons, it should be remembered that most of the children examined in the Pearson-Moul investigation were children of recent immigrants and 28·3 per cent. of them had been born abroad. Of the Jewish children tested in the present investigation only 3·1 per cent. were foreign born.

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[1] This subject is being further investigated by comparing the teachers' estimates with the test results.

[2] *Op. cit.* p. 126.

TABLE XIV. *Percentage Distributions of Intelligence.* (*Teachers' Estimates.*)

	Boys				Girls			
	Very able and capable	Intelligent	Slow	Dull and very dull	Very able and capable	Intelligent	Slow	Dull and very dull
School A:								
Non-Jews	7·8	34·3	35·0	22·9	9·1	25·0	31·1	34·8
Jews	21·5	42·7	25·4	10·4	13·9	32·3	23·5	30·3
School B:								
Non-Jews	13·9	36·5	23·5	26·2	7·0	26·3	24·6	42 2
Jews	45·2	35·5	11·6	7·7	26·9	31·6	20·5	21·0
School C:								
Non-Jews	5·2	38·3	33·0	23·5	3·7	20·1	27·6	48·5
Jews	16·9	49·3	26 4	7·4	15·5	48·4	25·4	10·7
Pearson-Moul data:								
Jews	16·0	36·2	34·7	13·1	7·5	25·5	37·8	29·2
London Pioneer Survey*:								
Non-Jews. Better schools	18·6	34·8	23·1	23·5	13·9	34·6	25·0	26·5
Medium schools	21·3	37·2	24·7	16·8	12·4	41·4	32·5	13·7
Poor schools	15·6	32·3	23·5	28·6	15·8	37·2	29·4	17·6
Gilby's data†:								
Non-Jews	25·1	31·0	23·6	20·3	—	—	—	—

* The original estimates were obtained from twelve London schools, the teachers using a five-point scale. The figures given here are quoted from Pearson and Moul, *op. cit.* p. 126.

† *Biometrika*, VIII, 94–108. These figures were obtained from a number of London schools, the teachers using Pearson's scale.

CONCLUSIONS.

1. On an average, both in General Intelligence and in attainments in English and Arithmetic, Jewish children are definitely superior to the non-Jewish children attending the same school, the superiority being more marked with the boys than with the girls. This superiority is found at every age from 8 to 13 and in every type of elementary school tested. The superiority in intelligence revealed by the tests is confirmed by the independent estimates of the teachers.

2. The 'racial' difference in favour of Jewish children amounts in the case of the boys to about one year in Intelligence and in Arithmetic, and to a year and a half in English at age ten. Among girls the differences are equivalent to a year in English and to slightly less than a year in Intelligence and Arithmetic.

3. Variations in the differences between Jewish and non-Jewish children are found at different ages, the most noticeable and the most consistent being a temporary drop at the age of 12 followed by a distinct rise at the age of 13. There is no indication in the data (which so far have been obtained only from children of 8 to 13 years of age) that the Jewish child's precocious development is followed by a premature arrest.

4. No sex difference in English or Arithmetic is found among non-Jewish children: Jewish boys are on an average superior in these subjects to Jewish girls (by the equivalent of about four months' progress at 10 years of age). In intelligence the Jewish boys are slightly superior to Jewish girls, but the contrary is found among non-Jewish children.

We wish to acknowledge our indebtedness to Professor Cyril Burt for much helpful advice and guidance which he has given us in connection with this investigation.

INFLUENCES OF CONATION ON COGNITION[1]

BY E. H. WILD.

(*From the Psychological Laboratory, University College, London.*)

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III. *Conation and Work* (pp. 148–149).
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V. *Conation and Motor-performance* (pp. 156–157).
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IX. *Summary* (p. 165).

I. INTRODUCTION.

THE problem of the influences of conation on cognitive processes has received inadequate attention at the hands of research workers. In this paper I shall attempt an account of some previous investigations, together with an explanation of their results and the conclusions to be drawn from them. I shall deal with results obtained from experiments carried out by myself in a subsequent paper. Throughout the whole discussion constant use will be made of McDougall's "hormic" theory (22) and Spearman's *Principles of Cognition* (41).

II. MEASUREMENT OF CONATION.

A lengthy search through the literature revealed only a few researches bearing on the very difficult problem of the measurement of conation.

H. T. Moore (25) attempted to measure the relative strength of the instincts of fear, anger, and sex-interest by measuring their disturbing effects on ability to carry out mental multiplications. He showed that disturbances varied in descending order of magnitude with the instincts of "fear, anger, embarrassment, and sex-interest." This variation is partly due to the fact that the respective stimuli were obviously not equally effective as excitants of the several instincts.

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[1] This paper is based on the first part of a thesis approved for the degree of Ph.D. of the University of London, December 1926.

H. T. Moore and Gilliland(27) attempted to measure the aggressiveness of an individual in terms of his ability to resist a type of distraction involving a certain element of fear. In general, the stronger the instinct of pugnacity in an individual, the weaker did his instinct of flight appear to be.

Morgan(29, 30) gives increased pressure on keys and increased breathing rates as evidence of increased conation in experiments on perception and learning. Dodge(11) attempted to measure effort in terms of the amount of metabolism in the neurons. Thorndike(42) criticized this adversely and attempted to measure it in terms of the 'satisfyingness' of the performance, a method which has grave difficulties in the way of accurate grading of 'satisfyingness.'

Wells(48), in psycho-galvanic reaction experiments, found "that a fall in resistance occurred just after the presentation of the stimulus.... The regular conscious antecedent to this psycho-physiological phenomenon is of a conational nature."

Interesting and suggestive as these researches are, they do not afford any satisfactory objective means of measuring conation.

III. CONATION AND WORK.

For completeness, this review will begin with experiments showing the effects of conation on muscular activity. In the experiments of Triplett(46) two subjects turned fishing-reels separately, first alone, and afterwards in each other's presence. The second situation produced an increased amount of muscular work. Wright(54) made his subjects use an ergograph (1) unmotivated, and (2) motivated by watching their strokes (*a*) while doing a task within their powers, and (*b*) while doing a task obviously beyond their powers. "The difference in the mental attitudes of the subject under the conditions imposed on him...affected in no uncertain manner the results accomplished by him." The incentive resulted in more work being accomplished with less fatigue, but the attempt to achieve a known impossibility resulted in a smaller increase than in the case of attempting a possible achievement. Arps(1) found in experiments with the ergograph that the amount of work and the rate of work when the subjects could see ergographic records exceeded the amount and rate of work when they had no knowledge of the results.

In these experiments extra conation is derived from the gregarious instinct and the self-assertive instinct; the effects of the increased conation are shown in the increased output. Evidence is also given of the reinforcing of conation by successful achievement and the diverting of it by failure.

Hence it is seen that increased effort (conation) stimulated by the appeal to instinctive tendencies results in increased output of muscular work.

IV. CONATION AND APPREHENSION OF EXPERIENCE.

Passing now to experiments that are included under Spearman's Principle of *Apprehensibility of Experience* ((41), Ch. IV), we find that these are confined to researches on 'sensation' (or sentient experience), usually under such headings as "The Effects of Attention on Sensation."

McDougall (22) has helped to disperse much of the confusion in the use of the term 'attention,' by showing that attention is merely conation considered from the point of view of its effects on the cognitive process. The fundamental condition of attending is that some cognitive process shall awaken a conative disposition and so arouse the impulsive energy of some instinctive tendency.

Attention thus conceived is entirely different from the 'clearness of sensations' with which it is often identified. The latter so-called 'attention' is really the degree of clearness of apprehension of sentience.

Non-voluntary attention occurs through direct stimulation of a powerful instinct or strong sentiment. When an object does not itself arouse a strong impulse but is attended to in order to attain some remoter end, the conation in respect of that end sustains our attending to the object; sometimes such attending is sustained by an effort of will, which derives from the intricately organized system of conative impulses comprising the whole personality. This 'voluntary attention' is obviously of the same conational nature as 'non-voluntary attention.'

Sensory perception, according to Prof. Spearman, consists first of all in pre-mental, and therefore pre-cognitive, events of a purely physical or physiological character, which result in a sentient state of mind or sensation—a mental, but still pre-cognitive event. This initial mental effect of sensory stimulation is taken to be "sensation in the strict meaning of a state" ((41), pp. 45–47). This merely lived experience comes within the scope of the Principle of *Apprehensibility of Experience.* While the intensity of the sentience is rising from zero to its maximum, the cognitive process may begin, viz. the apprehension of the nature and occurrence of the sentience ((41), p. 242). In the apprehension of experience then we have "three different characters rising to their maximum points separately"; viz. intensity of sentience ($\frac{1}{10}$ sec. for vision), intensity of cognition ($\frac{3}{10}$ sec.) and determinateness (1 sec.). The first is pre-cognitive, but the two latter are the constituents of clearness of cognition. "It would seem that a person's conations can directly control his cognitions in respect only of intensity; but this in turn works upon their determinateness" ((41), p. 161).

Attention then is conation; it changes the intensity of cognition and a change in determinateness follows on this. These two changes then constitute the variation in clearness of cognition of the sentience produced by variation in attention.

The general method of research in this field has been by studying the effects of distractors, especially the effects on reaction-time. Now there are a number of factors involved in reaction processes as determinants of reaction-time. Among these are the following:

(i) *Conation.* The intention to react results in a cerebral adjustment which produces a transient sensori-motor arc. See James ((18), p. 90), Woodworth ((53), p. 485), Todd (45), Woodrow (52), McDougall (20), Spearman ((41), p. 162).

(ii) *The speed-efficiency of the sensori-motor arc temporarily formed by conation.* Variations in conation may produce variations in the speed-efficiency and hence in the reaction-time.

(iii) *The attainment of such clearness of cognition of the sentience as may be necessary before the efferent part of the arc begins to function.* This clearness of cognition depends on the degree of conation. The apprehension of the sensation may not occur at all if its intensity is below a certain degree; the sensation becomes more difficult to apprehend clearly as the intensity of sentience (which depends on physical intensity) approaches a minimal value. There is then a physiological threshold limitation on the occurrence of the reaction.

(iv) *The time of attainment of such cognitive clearness.* Conation appears to reduce the time needed for the attainment of maximal cognitive intensity ((41), p. 318), so that conation may affect reaction-time by speeding up the rise to the cognitive clearness necessary for reaction. This reduction in time is limited and hence there is a physiological time-limit to the speed of reaction. The physiological excitement accompanying conation may affect reaction-time (*a*) by reducing the physiological time-limit, and (*b*) by lowering the physiological threshold; both resulting in a reduction in reaction-time.

(v) *The intensity of the stimulus.* Greater intensity results in more rapid rise of intensity of sentience, and this in turn affects reactions which are sufficiently automatic to require little, if any, cognition of the sentience; see Sherrington ((39), p. 26).

(vi) *The degree of practice.* The effect of repeated conation in practising may result in the process becoming so nearly reflex that the reaction will follow directly on the sentient experience and before the apprehension of sentience has reached its maximal clearness. Then the conation merely maintains the reflex process and the times of reaction are physiological times.

It is obvious that since variation in reaction-time may arise from a number of causes, the results based on reaction-times are of a suggestive, rather than a conclusive nature.

An excellent survey of the work on 'Attention' up to 1909 is given by Geissler (15). He emphasizes the importance of the clearness factor and criticizes adversely current methods of measuring attention. Munsterberg and Kozaki (32) examined intensities of sensations produced by light, sound, lifting of weights, and by eye-movements, and obtained results which failed to be corroborated by Hamlin (17), who found that the distractions used acted as an incentive rather than as a check to the conation. Todd (45), investigating light, sound, and electric shock stimuli, obtained results which indicate that the addition of stimuli seems to produce a more rapid rise to cognitive clearness.

Woodrow (51) attempted to measure "the degree of attention" in terms of the reciprocal of the absolute prolongation of reaction-time produced by irregular preparatory time-intervals, as compared with regular time-intervals. Following Meads (24), he used a square of light and a formless spot of light of the same objective intensity. The results indicate that definiteness of outline is a condition of clearness of cognition of sentience. Irregular time-intervals lengthened the reaction-time because they prevented the readiness for reaction resulting from cognition of the time-interval. He also concluded (52) that "the degree of attention" was lower in choice reactions than in simple reactions—a statement arising from misuse of the term 'attention.' The increased reaction-time is due not to less attention (conation) in the choice-reaction, but to the fact that in choice-reactions the speed-efficiency of the reaction mechanism is necessarily less than in simple reactions.

Dallenbach (9) used attributive clearness as "a measure of attention to auditory stimuli," with distractions such as counting and adding; but the results showed only that differences in cognitive clearness were accompanied by differences in the ability to judge variations in the stimuli. He obtained the same results for experiments on cutaneous sensations (10).

Newhall(34, 35), in work on binaural sounds and on cutaneous sensations, held that varying degrees of attention were attained by arranging pre-stimulus situations such that the subject could judge with different degrees of accuracy when the momentary stimulus would occur. The author assumes that the 'attention' was lower when it was more difficult to predict the occurrence of the stimulus, whereas, if anything, the conation was then probably higher.

In all the above researches, the conation is great and practically constant throughout the experiments, no indications of conative changes being obtained. The effects measured are not changes in conation (attention) but chiefly changes in the degree of cognitive clearness necessary for the efferent apparatus to function under varied conditions, the conation throughout being maximal.

We will now consider those experiments in which means are taken to vary the conation, and in which some indications of the conative changes are obtained, as well as indications of the effects of such changes on the cognition of the stimuli.

Morgan(29) required his subjects to press a key corresponding to a given letter (or number) exposed, and records were made of their breathing and their pressure on the keys. The reaction-times were taken under quiet conditions and under conditions of auditory distractions by bells, buzzers and music. There was a general tendency to overcome the distractions by strain and articulation, and by these aids performance was maintained at an almost normal level. An improvement of quality of work was obtained by those who used articulation, but no such improvement by those who did not. No constant relation between length of reaction-time of distraction was observed. The general tendency, however, was to increase the output of energy so as to keep the time constant, independently of the varying conditions. The articulation, strained attitudes, and the great increase in pressure used to depress the keys under distraction all indicated the exercise of greater effort than under normal conditions.

Cassell and Dallenbach(7) worked chiefly with two subjects, obtaining a large number of reactions and introspections from them. An auditory distractor to an auditory stimulus was used continuously and intermittently. Their conclusions indicated that

(i) With no distraction, a passive attitude occurred, *i.e.* relatively little conation was necessary, producing a constant reaction-time of normal length.

(ii) With distraction, an active attitude occurred which produced a slow and variable reaction-time. This was an attitude of 'attending away from' the distractor, and involved great effort. The attitude of the subjects was found to vary, boredom entering in, and it was indicated that the "effect of distraction depended on the conscious attitude of the moment." When the distraction was prominent in consciousness, greater effort was needed to attend to the stimulus (cf. Evans, below); with practice the distraction came more and more vaguely to awareness, the active attitude faded into the passive attitude and the stimulus was reacted to with relatively little effort—the distractor no longer distracted.

Evans (12) investigated the effects of practice in overcoming distractions, and the transfer of such practice effects. He trained his subjects

(i) with visual distraction to a visual stimulus, and then tested them with auditory distractions; and

(ii) with auditory distraction to an auditory stimulus, and then tested them with visual distractions.

The practice resulted as above in an "ability to ignore non-essential stimuli"; this produced an "attitude of confidence, stimulating the subject to an active interest in the work." He concluded that it was the 'attitude' or 'adjustment' that made transfer possible, *i.e.* the increased power to ignore a distractor of one sensory modality through habituation to a distractor of another sensory modality.

Johanson (19) obtained interesting results in experiments on reaction-times to sound stimuli as affected by a certain incentive and by threat of punishment. The conditions of testing were:

(*a*) Normal—a normal series of reactions to a sound stimulus.

(*b*) With incentive—subjects were informed each time of their preceding reaction-time, which they were required to reduce.

(*c*) With punishment—subjects were forewarned that if they took longer to react than previously they would receive an electric shock.

He found that (i) expectant attention (to sound) reduced "the reaction-time on the incentive and punishment series"; (ii) "the factors of positive and negative incentive caused the state of keener attention to be maintained"; (iii) the limit of reduction depended "on the subject's training and on the type of the individual, as the only limitation is the physiological time"; (iv) the actual saving, though not permanent, by incentive was 6 per cent. (or 8σ) and by punishment 15 per cent. (or 20σ) of the normal time. He suggested that incentive produces a "state of much keener attention, which motivates reactions very much prompter than those normally obtained under quieter conditions. The fact that there is a motive present, stimulating the subject to react, works the same on reaction-time as it does in all other types of responses to stimuli ...it seems that fear is a very powerful facilitator."

That the conation of the subjects was greater during the incentive series is indicated by their premature reactions (especially during the punishment series), by their postural attitude, copious perspiration, nervous tension, and in their introspections.

In the experiments of Morgan, Cassell and Dallenbach, and Evans, the acceptance of the instructions by the subject results in a conative concentration of mental energy for the cognitive processes necessary to carry out the task. This concentration of mental energy for these processes tends to limit the occurrence of any other cognitive processes, in accordance with the *Principle of Mental Energy* ((41), p. 131), but if another stimulus occurs such that some other instinct (in these experiments, curiosity) is awakened, the mental energy will tend to be directed to the cognition of that stimulus in spite of the subject's intentions. A reinforcement of the instructions by an effort of will is then necessary to inhibit the impulsive force of the instinct aroused—this explains the effortful attending to the stimulus under distraction. Attending under distraction is then achieved chiefly by inhibiting refractory impulses, and this is done by two methods:

(i) All available mental energy is employed in such processes as articulation, and organic energy in muscular reactions, leaving as little energy as possible for any other process; and (ii) by an effort of will, which is the combined action of instincts organized in character. Here we are brought to the question of whether inhibition may not occur directly, as well as indirectly by drainage. The drainage theory ((20), Ch. VII) can explain most of the phenomena, but it fails to cover satisfactorily the phenomena of selective inhibition, *e.g.* cases in which we inhibit thinking of some particular item without inhibiting thinking of closely connected items. Further investigation of this difficult question is necessary. McDougall ((20), p. 134) says that "if this view (drainage) is untenable, we should be compelled to look for an explanation in some psychical guidance of the neural discharge." Bearing the possibility of direct inhibition in mind here and throughout this paper, the drainage processes indicated above appear to go most of the way to explaining the phenomena.

In connection with practice effects, (*a*) the more often the distraction occurs, the less likely is it to arouse the instinct ((22), p. 273) of curiosity (as in these experiments); and (*b*) the more often the impulse is inhibited relative to a particular stimulus, the less strongly does it tend to be aroused by that stimulus ((22), pp. 191, 268); and (c) "to sustain our attention by...an effort of will (is)... a task... which is possible only as we have disciplined and practised ourselves in such efforts" ((22), p. 279). These processes combined explain the decrease in effort resulting from habituation to the distractor.

Finally it is to be expected that since the greatest conation is exercised in the service of the strongest instinctive tendencies, the surest way of producing strong conation is by appealing to strong instincts; for then the subject strives greatly to satisfy the instinct aroused whether the appeal is by a positive incentive or by a punishment. In reaction experiments the subject will strive harder to react quickly in order to satisfy the instinct, and 'fear' being more strongly appealed to in Johanson's experiments than the self-regarding sentiment, the effects of the former were considerably larger than those of the latter. Whether the effect of the increased conation due to the excitement of an instinctive tendency in reducing reaction-time is by speeding up the time of attaining the necessary cognitive clearness or by facilitating the efferent and motor part of the reaction process, or by both, is not indicated. The experiments do, however, seem to give support to the views suggested above as to the probable ways in which strong instinctive conation may affect reaction-time.

It is to be noted that improvement in reaction-speed occurs because such improvement is necessary to the satisfying of the instinct. If the instinct is strongly aroused without relation to the reaction-time the mental energy would have been conatively directed away from the reaction process, and the speed of reaction would probably decrease.

Conclusions. The researches indicate that apprehension of sentient experience is affected and effected by conation; sensations seem more intense as they are more clearly cognized; and cognitive clearness of the sentient experience is dependent not only on the intensity of the stimulus and of the sentience, but also on the conative control of the intensity of cognition of the sentience. The conation increases the intensity of cognition in accordance with the *Mental Energy Principle* ((41), p. 131), through direction of mental energy to such cognitions and away from other cognitions either in the services of some instinctive tendency, or by an effort of will; (possibly also conation may increase the intensity of cognition by directly increasing the absolute amount of available mental energy).

That the same results should be found for the rest of the material of the *Apprehension Principle* ((41), Ch. IV), viz. in the apprehension of cognitions, conations, and affections, as have been found for the apprehension of sentience, seems to be beyond doubt.

V. CONATION AND MOTOR-PERFORMANCE.

In considering motor-performance we pass to the fields of Spearman's Second and Third Principles of Cognition in their concrete manifestations ((41), Chs. V, VII). For here we have to deal with Eduction of relations and correlates with respect to Space and Time, together with muscular co-ordinations in carrying out the movements in accordance with the spatial and temporal eductions.

Tinker (43), using two grooved mazes, carried out experiments of three kinds, (*a*) without distraction, (*b*) with unexpected distraction, (*c*) with expected distraction. The distraction was the ringing of a bell whenever the sides of the grooves were touched. The results showed greater precision with the distraction, due to greater effort; the increased conation resulted in improvement in the motor performance through increased precision (eduction) and increased steadiness (muscular control).

Gates and Rissland (14), with a test of motor-coordination (Three Hole Test), obtained results that indicate a tendency to improvement with increased conation in motor-performance.

S. A. Courtis (8) showed the effect of conation on performance in the handwriting of school-children, and suggested two laws of variation of performance: (1) the law of adjustment, by which rate and quality vary inversely for constant conation; and (2) the law of effort, by which variations in the sum of rate and quality are taken as measures of variation in effort. The appeal of the teacher and money rewards were used as incentives to increased effort. The validity of the conclusions arrived at is difficult to gauge, because though the results show wide individual variations, statistical methods do not appear to have been applied to test their significance. Courtis regards "the evidence presented as proof that effort is a factor affecting performance in handwriting" ((8), p. 129) and finds "the same general situation exists" in the field of typewriting.

Conclusions. The above experiments suggest that in motor-performance conation produces:

(i) the more intense cognition of fundaments and relations in order to produce more accurate eduction of relations and correlates (for precision);

(ii) the eduction similarly of more accurate co-ordinatory relations between muscular response and the eductions of (i);

(iii) in complex performance greater control and co-ordination of the muscles themselves (*e.g.* in steadiness and in precision).

More complete and thorough investigation in this field is required before a conclusive analysis of the nature of improvement in motor-performance can be given.

VI. CONATION AND MOTOR-LEARNING.

Motor-learning is always accomplished in the service of and for the use of some instinctive tendency. "We learn...by purposive effort oft repeated to combine elementary movements in new ways...until it (the new combination) is, as we say, secondarily automatic" ((22), p. 179). Motor-learning or the acquisition of skilled movements is then the formation of motor-habits by repeated conation. Gopalaswami (16) analysed the cognitive side of motor-learning into the eduction of relations, the eduction of correlates, and associative reproduction.

No investigation seems to have been made as yet into the quantitative effects of increased conation on the speed, quality, or retentivity of motor-learning.

VII. CONATION AND MEMORY.

On Spearman's Principles, memory loses its unitary character as a faculty and takes its place as a manifestation of the same fundamental principles as account for the other cognitive processes ((41), Ch. XIX). Remembering involves as an essential condition a localization (implicit or explicit) in past time; the reproduced item differs from the originally cognized item in that it is now apprehended as belonging to the past. The concept of the past is arrived at, in accordance with Spearman's Principles, through a perception of 'nowness' as a character of experience and the perception of sequence as a relation educed between such characters.

Recall of the past may be explained either

(i) as a correlate eduction to reach the required portion of the past, closely followed by a reproduction of the required portion itself; or

(ii) as an intensifying of the subconscious presentation of one's past experience, which is always present, however faintly. This intensified past is next submitted to an explorative process. Reproduction then renders the apprehended past more complete in the diffuse manner of numerous multiple reproductions. From these a choice is made, through identification by inference of the reproduced item with the notion in mind.

The eductive processes completely determine the nature of new cognitive items, but cannot in themselves determine what previously cognized items shall re-occur. The *Principle of Mental Energy*, since it requires the simultaneous mental output to be a constant, explains how *any* item cognized in the past can possibly be evoked at all. The *Principle of Retentivity* ((41), p. 132) explains why, under the action of the Principle of Mental Energy, *certain* previously cognized items should reappear and not others. In order to explain the fact that previously cognized items actually do reappear in accordance with these two Principles, it is necessary to take account of the conative factor in recall.

There are two distinct ways in which conation plays its part here: (*a*) conation may affect the learning of matter in such a way as to determine its subsequent reproducibility, and (*b*) conation may have an influence on that subsequent process of actual reproduction. These two processes will be considered separately.

(*a*) *Conation and Reproduction.*

The importance of conation in the reproducing process is well illustrated in the introspective work of T. V. Moore (28). "Recall depends upon a definite mental mechanism"; but "the recall is in no sense of the word a purely reflex phenomenon" (cf. Bergson, below, p. 159), "but the process is initiated and directed by voluntary effort." Put into terms of Spearman's Principles, these introspections show that in the reproducing process conation directs the mental energy to cognition of the past by inhibiting as far as possible all other mental processes. Hence previously cognized items reappear and are cognized, in accordance with the *Mental Energy Principle*; the *Retentivity Principle* determines which of all one's previously cognized items they shall be. The conation produces its effects apparently:

(i) by inhibiting other processes so that more cognitive items will reappear; and

(ii) by directing mental energy to the more intense cognition and identification of the reproduced items.

Further, effects of conation working subconsciously for recall are shown in the spontaneous recall of items which some time previously we wanted to recall but which then had 'slipped our memory.' Negative effects of conation on the reproducing process are found in mental pathology in cases of functional amnesia or forgetting of the past, induced by repression or shock. Here aversive conation may prevent recall in spite of the greatest voluntary effort. Further discussion of this can be found, *e.g.* in McDougall's *Abnormal Psychology* ((23), Chs. XII, XXIX).

That persistent and long-continued effort to recall results in reproduction of more and more of the past is evidenced in the technique of the psychotherapist, where by effort a great amount of the 'forgotten' past is recalled. The effects of conation (largely subconscious) in reconstructive recall are shown in the experiments of Myers and Myers (33), where the subjects spent a week or two in successfully recalling and reconstructing passages they could quote readily some years previously, but which in the meantime they had apparently completely forgotten. Thus one subject recalled 65 per cent. of "The Wreck of the Hesperus" not recalled for 5 years; another recalled 132 lines of a 182-line poem after 5 years.

(b) *Conation and Learning-for-reproduction.*

Bergson ((3), Ch. II), cited by Smith and McDougall (40), draws a distinction between 'habit memory' and 'pure memory': "the memory of a lesson remembered in the sense of learned by heart...has all the marks of a habit...the memory of each successive reading has none of the marks of a habit" (cf. T. V. Moore (28), p. 26). The latter authors found correlations to exist between the learning of nonsense-syllables and the learning of motor habits; but no correlation between these and the number of elements remembered of a picture seen once or the number of ink blots recognized after being seen once, though this latter pair correlated as highly as the former pair. 'Habit memory' they thus held to be different from 'pure memory.'

Now the reproducibility of nonsense-syllables depends almost entirely on the *Principle of Retentivity* as manifested in bare facilitation and inertia, where frequency and recency of cognition determine the tendency of the syllables to re-occur. This facilitation is, however, just as essential a pre-requisite in cases of 'pure memory.' In the latter the material is meaningful; it is rich in associations. This demands frequent cognition in the past of this material as related to other mental objects, so that its greater reproducibility is due to facilitation as before. The only difference is that in 'habit memory' all the repetitions take place in a short period in the laboratory and can be counted, whereas in 'pure memory' there have been innumerable repetitions spread over a long period of previous experience outside the laboratory. Hence 'habit memory' and 'pure memory' reduce ultimately to the same thing.

The researches now to be reviewed are those which aimed at discovering what effect conation in learning had on the subsequent reproducibility of the matter learnt. In general the subjects learnt with no intention to recall and also with intention to recall the matter learnt; so that they cognized (*a*) with little conation, or (*b*) with great conation in the learning process. The actual reproduction was done always with the same great conation.

Panicelli (37) used two homogeneous groups of 60 children each, so that one group learnt with intention to recall, and the other with no intention to recall. The same story was read to both groups and the effect of the 'will to learn' on the recall of verbs, nouns, etc. was found. The increased conation resulted in greater reproducibility of those parts of speech most frequently used by children and of words richest in association for them. The results cannot be relied on quantitatively, because of the omission of statistical criteria, but they show that voluntary effort in children from 8 to 13 years increases the reproducibility of familiar words learnt more or less incidentally in a context.

Mulhall (31) worked with two groups of students. Group A had (i) to arrange nonsense-syllables in four piles according to ease of articulation; and (ii) to arrange named photographs in piles according to attractiveness. They thus cognized the material with no intention to recall. Group B cognized the same material in the same way but "with a determination to remember." Immediately afterwards they were tested. The author gives no criteria by which the general equality of the two groups can be estimated, so that arguments from one group to another may not be valid. Subject to this criticism, the results indicated that conation in learning has a greater effect on reproducibility in proportion as the material is rich in associations (nonsense-syllables, names, photographs) when there is no time allowed for repetitive facilitation.

Brown (6) suggested that an improvement in the number of common advertisements recalled in later experiments by subsequent groups of subjects was "the result, perhaps, of the interest aroused (in the College generally) by the first experiment" with the first group of subjects.

In the experiments of J. Peterson (38) two classes learnt two lists of words. Class I learnt list 'a' with intention to recall, and list 'b' without intention to recall; while Class II learnt list 'b' with intention to recall and list 'a' without intention to recall. They were asked to recall both lists immediately afterwards, and then again (unexpectedly) four days later. The results, whose significance is not given, showed an increase in reproducibility of 15 per cent. and 30 per cent. for the respective classes for immediate recall, and 48 per cent. and 51 per cent. for the delayed recall. "The active attitude differs from the passive attitude not only in making the stimuli more intense: there is evidence also of better association in the active attitude." These statements are supported by the introspections given.

H. T. Moore (26) investigated the effects of lecturing with or without notes. Under competitive conditions two groups, A and B, showed no difference in a logical memory test (average scores, A = 40, B = 41·6), though the material was read to A and spoken without notes to B. The conation due to competition here swamped other conative effects. When the material was introduced casually into a lecture with the respective groups, the recall from A to whom it was read was 49·6 (m.v. 14·4), and the recall from B to whom it was spoken without notes was 67·5 (m.v. 15·7). There were greater interest in the lecturer and greater attention due to instinctive reaction, when he was unhampered by notes; so that the difference in reproducibility of meaningful material here was attributable to difference in conation aroused by the different methods.

Smith and McDougall (40) worked with nonsense-syllables with themselves as subjects. They obtained quantitative evidence of the ability to adopt extreme conative attitudes in the learning process. Thus with the passive attitude (little conation) one learnt the rows of syllables in an average of 45 repetitions; but later, when the attitude was more completely passive and the conation nearly zero, 204 repetitions were required. The other method required 39 and 165 repetitions respectively. With such experienced observers we have here reliable data, though for conclusive quantitative generalizations similar investigations with a large number of subjects are necessary.

No. of experiment	Subject	A		B		No. of row
	Degree of effort...	Max.	Min.	Max.	Min.	
1	To learn	13	89	9	100	1
2	To learn	10	41	7·5	90	2
	To relearn after <24 hours	6 (1 7)	4 (10·2)	4 (1·9)	5 (18)	3
3	To learn	9	99	13	90	4
	To relearn after 24 hours	4 (2 2)	5 (19·8)	8 (1·6)	7 (13)	5
4	To learn	10	40	8	172	6
	To relearn after 7–18 days	6 (1·67)	9 (4·4)	5·6 (1·4)	7 5 (22 9)	7

The table gives the averages for 20 rows (tabulated by the writer) of the number of repetitions required to learn one row of nonsense-syllables, but no dispersion values are given by which to judge their significance. The results indicate that conation in learning nonsense-syllables increases their immediate reproducibility as shown by the scores for each subject in rows 1, 2, 4 and 6. To compare the number-of-repetitions-to-learn with the number-to-relearn-after-an-interval, the former is divided by the latter (see bracketed figures). By comparing these bracketed figures it becomes clear that the reproducibility after an interval of time due to learning with maximal conation is greater than the reproducibility after an interval of time due to learning with minimal conation. The authors state: "We have illustrated the great importance of effort or volition in rendering repetition effective in memorizing."

Morgan (30) tested the effects of distraction during learning on the subsequent reproducibility of the matter learnt. A number and a three-letter-word were exposed together, ten such pairs constituting a set of paired associates. The subjects had to react to the word by pressing a key marked with that word's associated number. Four sets were learnt, and two days later the subjects were asked (i) to recall, and (ii) to recognize as many words as possible; and (iii) to react as before to the word by pressing the correct key. Two sets were learnt in quietness and two under auditory distraction. With distraction the subjects exerted greater effort as indicated by key pressure, breathing, and articulation, but as they began with great effort this limited extra conation was not able to overcome the distracting effects of the noises. Hence the cognition of the necessary relations was not so effective as before, the associative links formed under distraction were weaker, and the ability to recall and recognize lowered. This was more marked in delayed than in immediate recall, because with the fading of the recency factor (inertia), the associative factors become more important as determinants of reproducibility.

Boswell and Foster (4) carried out experiments with four subjects to find what effect was produced on learning by the "intention permanently to retain"; they used eight sets of paired associates, each set containing 16 pairs of words, each pair being one English word and its Chinese equivalent (in English characters). They learnt four sets for temporary and four for permanent memory and were tested with all the material five minutes afterwards and then again a fortnight later. It was found from introspections that the subjects depended on the inertia factor for immediate recall, and on association by eduction of numerous relations for permanent retention. The authors were convinced that the conation was constant in both cases, but the conative direction of the cognition was changed as indicated.

Conclusions. The evidence advanced leads to the conclusion that increased conation in learning results in increased reproducibility. The effect of increased conation on apprehensions, *ceteris paribus*, is an increased intensity of cognition. This may result in more intense cognition of fundaments (*e.g.* in learning nonsense-syllables), or of relations (*e.g.* paired associates), or of both (words and context of a passage). The differences in cognitive intensity according as one intends to remember the words or context of a passage are easily introspectible, and the difference in 'method' is immediately shown in the results in the greater reproducibility of the material which has been more intensely cognized. The increased conation in learning results in increased reproducibility because of the increased intensity of cognition produced by the conation. The conation produces increased intensity of cognition as indicated above (p. 150).

(c) *Feeling and Reproducibility.*

The experiments of Wohlgemuth (50) showed that "there is no difference between... pleasure and unpleasure in their influence upon memory," but these conclusions were based on experiments on the recall of pleasant and unpleasant recent experiences as such; and the equivalent reproducibility arises from the fact that all the experiences (pleasant or unpleasant) had been of about the same intensity. It is not affection, but the degree of conation and the intensity of the cognition in the original experience that determine reproducibility, as shown above. Thus when the conation in the original experience has been very intense or when the conative impulse is very insistent, then the experience has a strong tendency to reappear in the cognitive field. If such an intense experience is unpleasant, it will be a source of constant mental pain, which tends to divert the conation connected with it. The whole experience, together with all its associations, may be repressed, in which case it cannot be recalled. Conversely, an intense pleasant experience tends equally to recur, but the conation is here reinforced and the reproduced experience is frequently cognized intensely. In this way, by diverting or reinforcing conation in the original experience and on subsequent occasions of recall, affection has an indirect influence on reproducibility of previous cognitions.

Fox (13) gave 24 subjects four sonnets to learn, and showed that subjective preference for a sonnet had considerable influence on its reproducibility; there was an increased reproducibility of what was more pleasant to learn. This supports the views advanced above.

Except in so far as affection influences the conative and hence cognitive intensity of the original experience, it has no effect on the tendency of a *recent* experience to be reproduced. Over large periods of time with equally intense experiences, we would expect pleasant rather than unpleasant experiences to be remembered. For conation resulting in recall of a painful experience tends to be diverted, so that the cognition of it is cut short; thus the painful experiences are less often and less intensely cognized again, and tend in time to be forgotten in accordance with the *Principle of Fatigue* ((41), p. 134). Conversely, from facilitation, the pleasant experiences tend to be remembered.

(*d*) *Summary of Work on Conation and Memory.*

The experiments in general substantiate the view:

(i) That the increased conation during the cognition of experience (*e.g.* in learning) results in greater reproducibility of such cognitive items through more intense apprehension of them and of their relation to other cognitive items simultaneously cognized or reproduced.

Corollary. For a given amount of repetition in learning an increase in conation in learning increases reproducibility in proportion as the item cognized is rich in associations for the learner.

(ii) That affection only influences the reproducibility in so far as and because it reinforces or diverts the conation.

(iii) That conation in the reproducing process determines the reproduction in accordance with the *Principles of Retentivity and Mental Energy* ((41), pp. 132–5).

Corollary. The more intimately a cognitive item is associated with a strong sentiment the greater is its tendency to be reproduced.

VIII. CONATION AND INTELLIGENCE TESTS.

A. Bronner (5), in dealing with delinquents, reports the cases of about 20 subjects, many of whom had been tested ordinarily and graded as feeble-minded or sub-normal. She found that the performance of these subjects in mental tests was being adversely affected by the impulsive force of some such instinct as flight, pugnacity, submission, etc., with the result that when this adverse influence was removed and the attitude of the subjects was normal, they graded as much as three years higher in the Binet-Simon tests.

The necessity of arriving at some means of measuring the amount of conation a subject is directing to the performance in intelligence tests thus becomes apparent, but very little experimental work has been done here. In Bronner's work, however, the detrimental effects of aversive conation on performance in intelligence tests are very clearly demonstrated.

IX. SUMMARY.

In this paper I have reviewed some of the experimental work that has already been done on the influences of conation on cognition. Evidence has been adduced to show that conation does influence muscular output and cognitive output. On the basis of the Hormic Theory of Action and in the light of Spearman's Principles of Cognition, these researches lead to the following conclusions:

(1) By directing mental energy, conation produces more intense cognition of fundaments and relations or both (*Mental Energy Principle*); this makes the eductive processes more accurate and increases reproducibility of the items thus cognized.

(2) By directing or concentrating mental energy, conation causes previously cognized items to be reproduced more rapidly and numerously (*Mental Energy Principle*). The particular items that do reappear are determined by the *Retentivity Principle.*

These conclusions were arrived at from a study of the influences of conation on muscular work, on apprehension of sentience, on the efficiency of motor-performance, on the learning and reproducing processes of memory experiments, and on performance in intelligence tests.

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(*Manuscript received* 10 *May*, 1927.)

THE NEURAL BASIS OF PURPOSIVE ACTIVITY

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XIV. *Summary* (pp. 187–188).

I. INTRODUCTORY.

THE activities of men and of animals, whether in thought or in behaviour, are usually directed towards a goal or end; and this peculiarity of activity in which a mind is concerned has constituted a difficulty for physiological interpretation. The simple theories of the physiological basis of association fail to explain the purposive nature of our thinking, and the simple 'chain reflex' and 'conditioned reflex' theories fail to explain the purposive nature of animal behaviour.

There have, however, been several suggestions—none of them worked out in detail—towards a satisfactory theory of the neural basis of purpose. James[1] suggested that in directed thought (as contrasted with mere reverie) there is one brain tract which remains active as long as the thoughts are directed to a particular end, and that the activity of this brain tract has the effect of insuring that only relevant associations are aroused. MacDougall[2] has put forward the theory that underlying each instinct there is a powerful group of neurones (probably in the optic thalamus) and has applied this theory to certain phenomena of sleep, hypnosis, and repression. Woodworth[1] has advanced a similar view of the neural basis of all active tendencies, supposing that when any tendency is directing thought or conduct there is a specific brain tract which remains active until the goal of the tendency is reached or some other tendency is aroused.

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[1] *Textbook of Psychology*, p. 271.

[2] *Outline of Abnormal Psychology*, Chs. v, xi.

In the present paper an attempt is made to develop and modify these suggestions as far as seems necessary to explain certain well-known facts of human and animal behaviour, namely, the variation of behaviour until a goal is attained, and the simpler kinds of 'learning by experience.' The application of the theory so reached to certain more complex psychological processes is then briefly considered. Since, however, 'physiological psychology' of this kind has lately become unpopular both with psychologists and with physiologists, it is perhaps necessary to say a word about this more general subject before going on to consider the special case of purposive activity.

II. PHYSIOLOGICAL INTERPRETATIONS IN PSYCHOLOGY.

Any attempt such as that which is made in this paper raises two questions; firstly, whether it is theoretically possible to give an adequate explanation of behaviour in physiological terms, and secondly, whether physiological theories may profitably be suggested as the result of psychological study, *i.e.* as the result of the study of behaviour or of conscious processes.

The answer to the first question depends on the relation between body and mind, into the discussion of which it is not possible to enter here. I shall assume (admitting that the validity of the assumption is open to criticism on philosophical or on empirical grounds) that the relation is such that an adequate physiological interpretation is possible. I do not mean by this that physiological interpretation is the only adequate interpretation which may be given, or that it is any truer than an explanation in mental terms. All that is assumed is that, accompanying the mental processes, there is a continuous series of physiological events taking place purely according to physiological laws. This assumption is common to epiphenomenalism, psycho-physical parallelism, behaviourism (in those forms of it in which the existence of consciousness is admitted at all), and to the theory of emergent evolution, but it would not be admitted by those who hold some forms of the interaction theory.

On this assumption the behaviour of an animal may be regarded as the result of the working of its brain, and it is to be expected that the study of behaviour will throw light on the physiological processes which produce it. It is true that the study of the brain is usually approached by physiologists along other lines; but it seems likely that the results of observation of normal behaviour, as well as the results of observation of the disorders of behaviour produced by injuries to the brain, will have to be taken into account in reaching a true theory of cerebral physiology. This view suggests that physiological theories may be put forward as the result of the study of behaviour (provided, of course, that due weight is given to the relevant discoveries of physiologists using other methods) with as much chance of usefulness as theories ever have in any branch of science. It need hardly be added that until such theories have more direct evidence in their favour (such as evidence from the anatomical study of the brain or from observation of the effects of lesions) they must be regarded only as tentative working hypotheses.

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[1] *Psychology*, Ch. II.

Similar arguments appear to be applicable to theories of the physiological accompaniment of conscious processes put forward as the result of introspection.

III. Some Characteristics of Purposive Activity.

Let us consider some of the objective marks of purposive behaviour which distinguish it from the action of an inanimate body. Prof. MacDougall[1] has formulated these marks as the "marks of behaviour," and it will be convenient to follow his order of treatment, considering in turn the neural structures and processes implied by each characteristic.

The first five marks of behaviour distinguished by Prof. MacDougall are:

(1) A certain spontaneity of movement.

(2) The persistence of activity independently of the continuance of the stimulus which may have initiated it.

(3) The variation of direction of persistent movements.

(4) The coming to an end of the animal's movements as soon as they have brought about a particular kind of change in its situation.

(5) Preparation for the new situation towards the production of which the actions contribute.

The first, second, and fifth of these marks describe obvious ways in which the actions of an animal seem to an outside observer to be different from those of an inanimate object; but it is not claimed by anyone that these three marks by themselves form very serious obstacles to current neural theories. It is, therefore, unnecessary to consider them in detail. Prof. MacDougall admits, in discussing the first, that it is possible that "every instance of behaviour is initiated by a 'stimulus,'" and it may well be argued that in any case there does not seem to be any good physiological reason for denying that trains of action may be initiated by some more or less lengthy process taking place within the central nervous system without the intervention of sensory stimuli, though we can at present only guess at the nature of such processes. And the second and fifth marks of behaviour may be explained even on the crude chain reflex theory.

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[1] *Outline of Psychology*, Ch. II.

IV. BEHAVIOUR CYCLES.

The third and fourth marks are a great deal more important from the point of view of this paper. The facts described by them, which have long been recognized as evidence of the activity of mind, seem to be quite inexplicable on the simple chain reflex theory. It will, therefore, be necessary to consider these facts in some detail.

The behaviour of an animal is varied within certain limits until a certain situation is attained, and the behaviour then ceases. That is, there is carried out a series of trains of action A, B, C... until a certain situation E is attained (provided, of course, that the series is not interrupted by accident, fatigue, or the arousal of a tendency to perform some other series of actions); and when E is attained, whether early or late in the series, the behaviour is discontinued. It is not meant by this that the animal necessarily becomes quiescent when E is attained, for some other tendency may then be aroused. What is meant is that if E occurs immediately after A, then B, C... are not performed; if E occurs immediately after B, then C... are not performed, and so on. Under these conditions the whole series of actions from A to E may be spoken of (in Mr Bertrand Russell's phrase[1]) as a 'behaviour cycle,' and the situation E which brings the cycle to its close may be termed the 'end' or 'outcome' (Lloyd Morgan) of the cycle[2]. We may also speak of the actions A, B, C... as being 'directed towards' or 'carried out in pursuit of' the end E.

V. AN ACTUAL BEHAVIOUR CYCLE AND ITS NEURAL BASIS.

As a concrete example of a behaviour cycle we may take the case of a hungry chicken looking for food. If a chick which has not been fed for some time is placed on a fairly uniform piece of ground, then (unless some other behaviour cycle such as that due to the gregarious instinct intervenes) the chick will probably walk about, continually looking round in all directions, turning sometimes one way and sometimes another, and occasionally pausing to scratch. When small objects of certain kinds come within its field of vision it pecks and swallows them; and when it has swallowed a certain number the chick becomes less active and finally (unless some new behaviour cycle now commences) it becomes quiescent.

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[1] *Analysis of Mind*, Ch. III.

[2] Mr Russell's word "purpose" does not seem suitable for use in this purely objective sense, since "purpose" is nearly always taken to mean an end which the animal foresees.

Consider first a hypothetical simplification of this behaviour in which the chick alternates between walking directly forward and pausing to scratch until it has found sufficient food to bring the cycle to an end. Suppose that the actions of walking forwards are due to the discharge in a certain order of a complex system of motor neurones which we will call *A*; and the actions of scratching to the discharge of another system *B*. Then it is clear that the neurones in such motor systems must be supposed to be connected to each other and to certain afferent neurones (*e.g.* in both these cases to afferent neurones stimulated by proprioceptive impulses from the legs) in a complicated manner, the details of which we do not at present know; but it is not necessary to know the details of these connections for our present purpose, since we are now interested not so much in the neural basis of walking and scratching as in the explanation of the alternation from one to the other which occurs when the chick is hungry[1]. The actions both of walking and of scratching seem, in the case considered, to be due to the fact that the chick has not been fed for some time (though it may be that on other occasions they may form part of some other behaviour cycle besides that of hunger). It is an obvious suggestion that there is, somewhere in the chick's nervous system, a group of powerful neurones *E* which tend to become excited when the chick has not been fed for some time, and which are connected to the motor systems *A* and *B* in such a way that the excitement of *E* tends to excite *A* and *B*. I shall refer to such neurones as '*E*-cells.' This arrangement is shown in Fig. 1, where, as in all the other diagrams in this paper, one triangle represents a group of functionally similar neurones. The alternation between walking and scratching may then be explained in a way exactly analogous to the well-known explanation of alternating reflexes[2]. That is, it may be supposed that there is a relation of reciprocal inhibition between *A* and *B*, so that, while *E* tends to excite both, it can in fact only excite one at a time. Then if (owing, let us suppose, to the normal resistance of the path *EA* being less than that of the path *EB*) *E* at first excites *A* more strongly than *B*, the activity of *A* will inhibit that of *B* and the chick walks forward. But after a time the system *A* or some part of the path *EA* becomes fatigued (or enters its refractory phase), and the activity of *A* and its inhibitory action on *B* become less, until *B*, which is all the time being stimulated by *E*, becomes active and, in turn, inhibits *A*.

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[1] Such systems are what are referred to by MacDougall (*Outline of Psychology*, pp. 114–118) as "motor mechanisms" which may be employed by various instincts. Much light has been thrown on the nature of such motor systems by the researches of Sherrington, Head, and others.

[2] Cf. Sherrington, *Integrative Action of the Nervous System*, p. 200.

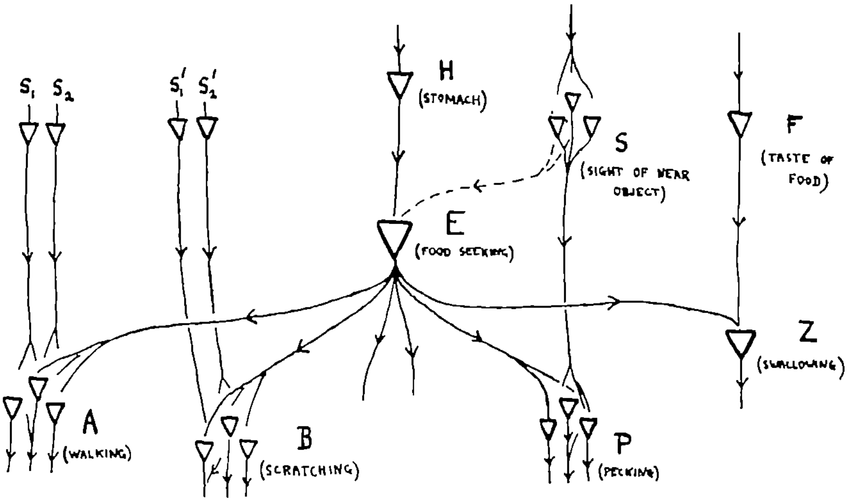

Fig. 1.

Nothing has been said so far about how the activity of one system can be supposed to inhibit that of the other. For many purposes it is convenient to picture this as taking place by a kind of "drainage," as in MacDougall's well-known theory, but it must be remembered that a great deal of recent work in physiology is difficult to reconcile with the drainage theory, and that many physiologists believe that inhibition is the result of a kind of "interference" of nerve impulses, and not of "drainage." To the theory given above it makes very little difference which of these views is true. All that the theory requires is that there should be systems such as *A* and *B* so connected that the action of one inhibits that of the other, and this is admitted by everyone. It is clear that this arrangement will produce an alternation between walking and scratching as long as *E* remains active; and if we suppose that the motor systems of the other actions of the behaviour cycle (such as turning to the right or left) are connected to E in the same way, the arrangement will produce variation from one to another of these actions until the end of the cycle is attained. But it is probable that the variation from one line of action to another is usually determined in a slightly more complex way. The motor systems (such as A and B) are usually if not always connected to afferent neurones ($S_1 S_2 \ldots S_1' S_2' \ldots$ in Fig. 1); and which of the systems A, B, $C \ldots$ becomes active at any moment is probably determined partly by the degree of excitement received from E (as considered above) and partly by the degree of excitement from such afferent neurones. If, for instance, a chick tends to scratch on one kind of ground more than on another, we must suppose that the system of afferent neurones stimulated by the contact of the foot with that kind of ground is connected to the motor system of scratching (*i.e.* to B); so that on that kind of ground the behaviour cycle will contain relatively more scratching than on another.

In the cases of pecking and swallowing such excitement of the motor system by a sensory system (stimulated by the image of a small object on the retina in the one case and by something in the mouth in the other) in addition to stimulation from the E-cells seems to be *necessary* for the action of the motor system to take place. In such cases the sensory stimulus may be called a 'key-stimulus' and the channel along which it is conveyed to the motor system a 'key-channel.' How such key-channels work is a problem which must be left to the physiologists: the fact that an animal will not carry out the actions of swallowing unless it is hungry and has food in its mouth is *prima facie* evidence for their existence. In Fig. 1 SP and FZ represent the key-channels of pecking and swallowing. Actions such as swallowing which (in this case after a number of repetitions) normally bring a behaviour cycle to an end have been termed "consummatory responses" (Sherrington) or "end reactions" (Woodworth).

It is now necessary to enquire why E tends to become active when the chick has not been fed for some time and quiescent after feeding. It is natural, in view of Cannon's well-known work on hunger[1], to suppose that E is stimulated by afferent impulses arising from contractions of the stomach. The contractions cease or become less pronounced when the stomach is full, and hence the E-cells become quiescent. In Fig. 1, H represents an afferent neurone which carries impulses from the stomach to E. It may be termed an 'interoceptive E-stimulator.' It is possible, also, that the sight of small objects stimulates E in addition to stimulating the motor system of pecking P. This possibility is indicated by the broken

[1] Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, Ch. XIII.

line *SE* in Fig. 1. In this case the system *S* would be an 'exteroceptive *E*-stimulator.' If it were shown that the sight of a small object increases a chick's general tendency to food-seeking as distinct from its tendency to peck at that particular object (*e.g.* if it were shown that the chick's tendency to carry out the other actions of the behaviour cycle was increased) this would be evidence for such a connection between the retina and the *E*-cells. A further question would then arise—whether such retinal connections to the *E*-cells are innate or acquired.

It will be seen that the essential difference between the theory given here and the chain reflex theory is that while on the chain reflex theory it is supposed that a motor system is excited only by sensory stimuli such as visual stimuli and stimuli from muscles which have just contracted, the present theory supposes that motor systems are excited also by certain central systems of neurones which we have called *E*-cells. That is, the *E*-cell theory supposes that in purposive actions the motor system is normally excited by at least two other systems, a sensory system (determined by the animal's present situation) and an *E*-cell system (determined by the end which the animal is pursuing). It will be suggested later that a similar kind of 'double innervation' occurs in directed thought (Section XIII).

VI. APPLICATION OF THE THEORY TO OTHER BEHAVIOUR CYCLES.

If the *E*-cell theory is true for hungry chicks it is probably true also for other hungry animals, the essential differences between the neural structure underlying hunger in a higher animal and that in a chick being in the complexity of the sensory and motor systems involved and in the number of 'secondary' (*i.e.* acquired) connections.

It seems probable, also, that a similar explanation holds for the variation from one line of action to another which is met with in other behaviour cycles than that of hunger; in fact it may be claimed that such variation seems almost necessarily to imply (if we assume that it has any physiological explanation at all) the persistent activity of one brain tract (which we have called '*E*') during the cycle, this tract being connected to the various motor systems in the way considered above. The persistence of *some* physiological process is at least implied and the simplest assumption is that this process is the activity of a certain brain tract.

But it seems likely that in some behaviour cycles (such as that due to the 'gregarious instinct') the activity of the *E*-cells during the cycle and their quiescence when the end is attained, is due to causes other than the presence or absence of impulses from an interoceptive *E*-stimulator such as that which we have supposed in the case of hunger; for in these cases we have no evidence at all of the existence of such impulse. It may be that further research will discover them, and in that case the explanation in other cycles may be similar to that in hunger. But it seems probable that in some cases the *E*-cells are normally so highly charged with nervous energy that they tend to become active without any sensory stimulation, or that they only require impulses from exteroceptive *E*-stimulators in order to do so. And the quiescence when the end of the cycle is attained may be explained by assuming that the discharge of the *E*-cells to the motor system of the consummatory response is of such great intensity as to drain off all the free energy from them, or by assuming that the afferent systems stimulated by the end situation have an inhibitory action on the *E*-cells which persists for some time after the stimuli have ceased.

If we consider such behaviour cycles as those due to the various human instincts we shall see that they probably differ in these respects. There is a great need for a detailed study of other behaviour cycles besides that of hunger from this point of view. In some cycles there is a further complication in the fact that internal secretions as well as neural connections play an important part in determining behaviour, but it is not possible to discuss any of these cycles in detail here.

VII. THE PROCESS OF LEARNING.

The sixth mark of behaviour distinguished by MacDougall is "Some degree of improvement in the effectiveness of behaviour when it is repeated by the animal under similar circumstances." This process of improvement in effectiveness takes place in several ways, two of the simplest of which are:

(1) Actions which in a given situation lead to the attainment of the end of a behaviour cycle tend to be repeated when that situation occurs again. This may be called 'positive learning.'

(2) Actions which in a given situation fail to lead to the attainment of the end of a behaviour cycle, or which lead to the occurrence of certain (painful) sensory stimuli tend not to be repeated when that situation occurs again. This may be called 'negative learning.'

Examples of these two kinds of learning are to be found in the behaviour of Lloyd Morgan's chickens, Thorndike's cats and dogs, and the animals in a Yerkes box.

VIII. THEORIES OF THE NEURAL PROCESSES IN LEARNING.

Several theories of the neural processes implied by such learning have been put forward at various times, of which the best known are perhaps those of James[1], Thorndike[2] and Watson[3].

James's theory is intended to explain why a baby who grasps a candle flame and is burnt does not grasp it a second time. The theory supposes that the sensory neurones S^1 (Fig. 2) stimulated by the sight of a candle become connected by association to the sensory neurones S^2 stimulated by the afferent pain impulses. This association is supposed to be so strong that on the second occasion when the baby sees the flame the excitement of S^1 excites S^2 rather than the motor neurones of grasping M^1 to which S^1 is innately connected; M^1 being, on the second occasion, 'short-circuited.' This theory applies only to negative learning.

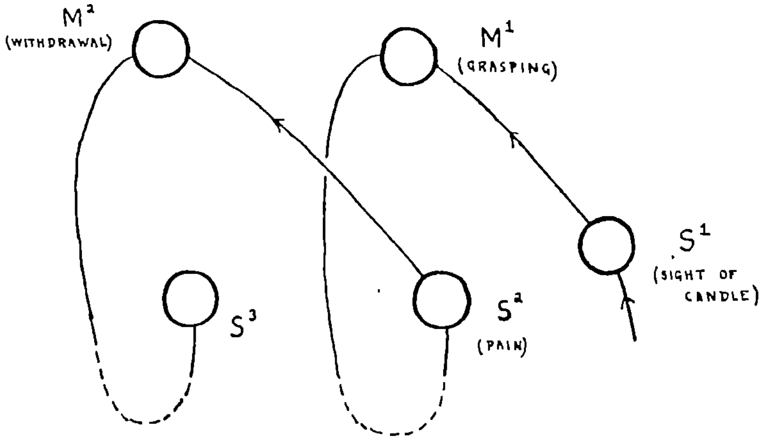

Fig. 2.

Thorndike's theory, on the other hand, applies to both kinds of learning. Thorndike supposes that pleasure leads to the stamping in of those responses which have led to it by somehow deepening the neural channels involved, and that unpleasure has an opposite effect. This theory is not a purely physiological one and it will not be discussed here.

Watson's theory supposes that successful responses tend to be repeated rather than unsuccessful ones because they are, on the whole, carried out more frequently. The process of learning is thus supposed to depend merely on the formation and strengthening of habits by 'conditioning.'

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[1] *Principles of Psychology*, II, 591.

[2] *Educational Psychology*, I, 172.

[3] *Behaviourism*, p. 257.

This theory has been shown to be inadequate by Peterson, who found that positive learning may still occur when both frequency and recency factors tend to produce negative learning (cf. Section IX).

Recently Lloyd Morgan[1] has put forward a more satisfactory theory. Lloyd Morgan takes the example of a chick learning to peck at ('nice') maggots with greater energy and not to peck at ('nasty') ladybirds; and supposes that in the former case the engram A' underlying the actions of seizing due to the excitement of the retinal pattern of the maggot (a' in Fig. 3 A) becomes connected to the engram of swallowing C', and that the connection is such that in future the excitement of A' excites

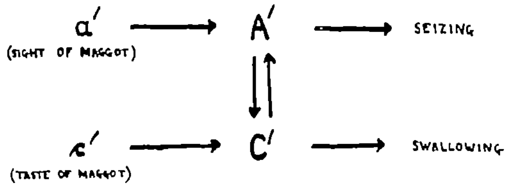

Fig. 3 A.

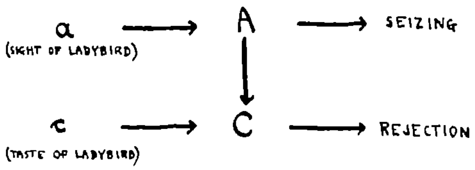

Fig. 3 B.

C' which in turn reinforces the excitement of A'. This connection is represented by the double line in the diagram.

In the negative case Lloyd Morgan supposes that the connection is only *from* A (Fig. 3 B), the engram underlying the actions of seizing, *to* C, the engram producing the response of rejection, and that in future A excites C rather than the primary pecking response. The explanation of negative learning is thus essentially similar to that of James, but the theory has the additional merit of suggesting an explanation of the difference between the positive and negative cases.

In the following sections an attempt is made to show that this theory fits in naturally with the E-cell hypotheses (C' and C in Figs. 3 A and 3 B being E-cells) and to develop the theory in somewhat greater detail than is done by Lloyd Morgan.

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[1] Lloyd Morgan, *Life, Mind, and Spirit*, Lecture IV.

IX. THEORY OF POSITIVE LEARNING.

It will be convenient to take as an example of positive learning the behaviour of an animal placed in an enclosure such as that shown in Fig. 4, as in the many well-known experiments of Yerkes. The animal starts at *O* and can see or smell some food which is placed at *X*. It can go towards this down either of the passages *L* and *R*, but *L* is blocked by a sheet of glass or a wire net, so that if an animal starts this way it has to return to *O* and then go to *R* before it can obtain the food. It is found that under these conditions the animal is at first equally likely to go to either passage, but that after a longer or shorter process of learning it goes to *R* more frequently, and that finally it always goes to *R*.

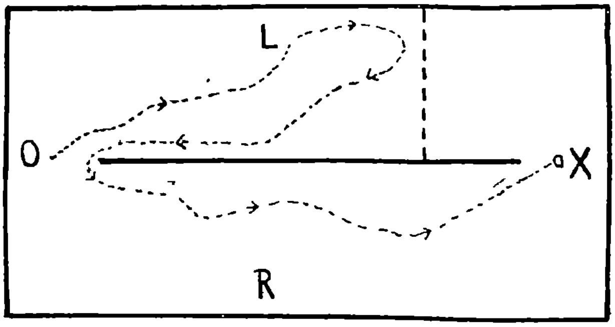

Fig. 4.

Suppose that the actions of turning to the left to go to *L* are produced by the discharge to the muscles of a system of motor neurones *A* (Fig. 5), and the actions of turning to the right to go to *R* by that of another system *B*. Since the actions of either system are only performed when the animal can see food at *X* and is hungry (or, at least, are performed with greater energy under these conditions) it may be supposed that at the beginning of the experiments *A* and *B* are both connected to the *E*-cells underlying the seeking of food and to the sensory systems stimulated by the sight of the food (*i.e.* to *E* and *S* in Fig. 5).

Then on the first occasion when the animal is placed at *O* the activity of *E* and *S* stimulates both *A* and *B* in some degree, which becomes active depending on the relative strengths of the channels to *A* and *B*, and on various accidental circumstances such as the exact position of the animal. Suppose that the animal goes to *L* in the first trial: then because neural currents flow from *S* and *E* to *A*, it is to be expected on the principle of neural habit[1], and because the activity of *A* accompanies or follows immediately that of *S* and *E*, it is to be expected on the principle of neural association[2] that the fact of the animal's going to *L* will strengthen the channels *SA* and *EA*. And since the activity of *E* accompanies that of *S*, it is to be expected that a channel *SE* will be formed or strengthened. Similarly, when the animal goes to *R* this fact will lead to strengthening of the channels *SB*, *EB*, and *SE*. It is therefore to be expected that on being placed at *O* the animal will tend to perform whichever series of actions it has carried out more frequently in the earlier experiments. Such a tendency undoubtedly exists, but the experiments of Peterson[3] show that this principle alone is quite inadequate to explain what occurs. Even if both frequency and recency factors tend to make the animal go to *L* rather than to *R*, the animal is found to go to *R* more often than to *L* (cf. Section VIII).

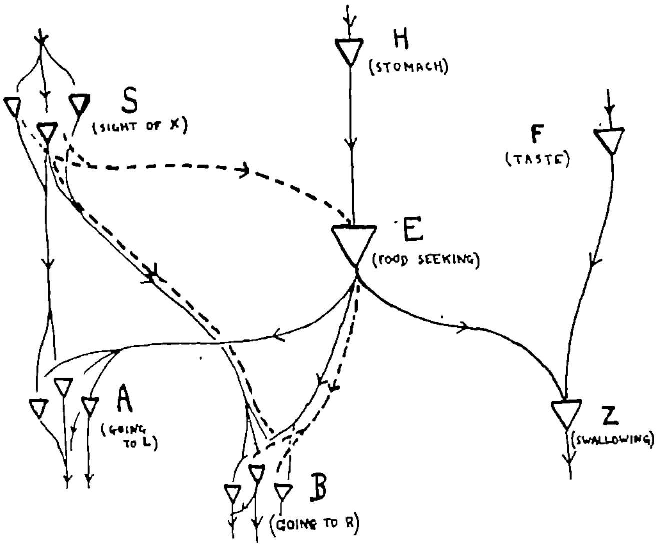

Fig. 5.

Broken lines indicate channels which are formed or strengthened during learning.

It is therefore necessary to assume that the important condition for the arousal of one of the systems *A* and *B* is not that its discharge shall most often have followed that of *S*, but that it shall most often have preceded that of *Z*, the motor system of the consummatory response. We have already seen that the consummatory response involves a discharge of nervous energy from E to Z, and that in some behaviour cycles this discharge may be of such intensity as to drain all the free energy from E (Section VI). Now it is to be expected on the principle of neural association, already referred to, that this discharge from E following upon the activity of B will form or strengthen a connection between E and B, and this will not occur in the case of unsuccessful actions such as those due to A. But to explain what occurs we have to suppose that this formation or strengthening of a channel between B and E takes place in what has been termed the 'reverse direction,' *i.e.* that although the channel is formed or strengthened owing to the discharge of B just before that of E, it is such that by it E can excite B. This is, perhaps, surprising in view of the experiments of Krestovnikoff[1] and of the present writer[2] who found that the channels studied by them could only be formed in the 'onward' direction. But what these experiments tend to show is that a sensory system which is excited just after an E-cell does not form a reverse connection with it, though such a connection (*i.e.* from a sensory system to an E-cell) may readily be formed in the onward direction. They throw no light on the formation of connections between an E-cell and a motor system whose activity precedes that of the E-cell; and in order to explain the occurrence of positive learning it seems to be necessary to assume that in this case connections *can* be formed in the reverse direction. We may suppose that, in the lower animals at least, channels are usually formed or strengthened in the onward direction only, but that E-cells have the special property of forming or strengthening reverse connections with motor systems whose activity precedes by a short interval the discharge of the E-cells to the motor system of the consummatory response. Whether this is due in some way to the unusual intensity of the discharge from E to Z or to some other peculiarity of the discharge is a question to which it does not seem to be possible to give a satisfactory answer at present.

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[1] Cf. MacDougall, *Physiological Psychology*, p. 125.

[2] MacDougall, *loc. cit.* p. 124.

[3] Peterson, *Journal of Experimental Psychology*, v, 1922.

Another possibility is that the formation of reverse connections between the E-cells and motor systems such as B does not depend on the discharge from E to Z at all, but on some other physiological process which occurs in the E-cells (or in cells closely connected to them) when the end is attained. For instance, it may depend on the arrival at the E-cells of certain specific sensory impulses, such as those due to the taste of food. Or it may depend on a discharge from the E-cells to the motor systems which produce those internal changes which are the bodily concomitant of pleasure. If it were possible to test these theories in some way; for instance by determining whether learning in the case of food-seeking depends on the occurrence of swallowing, the taste of the food, or the occurrence of the internal changes characteristic of pleasure, such an experiment would be of great value.

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[1] Krestovnikoff, quoted by Bentoff, *Brain*, 1924, p. 109.

[2] Grindley, *British Journal of Psychology*, 1927, XVII, 210.

Such strengthening of channels in the reverse direction will, on any of these views, only take place in the case of the successful responses; or at least will take place to a greater extent in the case of the successful responses than in that of the unsuccessful ones, since on the average the time between the unsuccessful responses and the attainment of the end is greater than that between the successful responses and the attainment of the end. The system *B* will therefore tend to become more strongly connected to *E* than will the system *A*; so that, assuming that in the past the animal has made the right and wrong responses equally often, it will in future tend more strongly to make the right ones. The fact that this selective tendency seems to be so very much stronger in most animals than frequency and recency factors shows that channels are strengthened more rapidly by reverse association of this kind than by neural habit.

The above theory (which is a development of that due to Prof. Lloyd Morgan, to which reference has already been made) seems to be consistent with a great many of the observed facts of positive learning. We should expect from it the gradual fall of the learning curves, rapid at first and then slower; since in almost any way in which we try to visualize the process of the strengthening of channels we must picture the strength of the channel as increasing gradually towards a limiting value. We should also expect the experimentally observed fact that rats learn the last part of a maze before the beginning[1], and the fact that Thorndike's animals were able to learn to perform actions which on their first performance were not carried out in pursuit of the end of the behaviour cycle in which the learning occurred (*e.g.* such actions as licking the right shoulder in order to get out of a cage[2]). Further, we should expect the general outburst of energy which occurs when a quiescent animal receives a sensory stimulus which has become associated with some behaviour cycle, as when a dog sees its master take down his hat and coat.

X. THEORY OF NEGATIVE LEARNING.

In the simple Yerkes box experiment considered above there is no need to assume any special mechanism for the inhibition of the actions of going to *L* except that due to the stronger tendency to go to *R*. But in some cases it has been shown that if an animal receives an electric shock on going to *L*, it learns more rapidly. To explain this we have to assume that there is some sort of mechanism for inhibiting actions which lead to situations in which (in consciousness) pain is experienced. Now in these situations we almost always find that the animal commences a behaviour cycle the end of which is the removal of the unpleasant stimulus. We shall therefore suppose that in an unpleasant situation the unpleasant stimuli excite *E*-cells which are connected to the motor systems of the various actions of escape, rejection, struggling, etc.; and we find, in accordance with what we should expect from the theory of positive learning given above, that an animal placed repeatedly in an unpleasant situation gradually learns more efficient ways of escaping from it.

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[1] Cf. Washburn, *The Animal Mind*, p. 225.

[2] Thorndike, *loc. cit.*

It therefore appears that in the behaviour cycles of these *E*-cells, those actions which lead to the initiation of the cycle become 'stamped out' (*i.e.* are not performed on future occasions), while those which lead to the attainment of the end of the cycle become 'stamped in.' We must therefore suppose that these *E*-cells have the following properties:

(1) They form reverse connections with motor systems whose activity precedes by a short interval the quiescence of the *E*-cells at the end of the cycle. This is the kind of connection discussed in Section IX.

(2) They form connections with motor systems whose activity precedes by a short interval the excitement of the *E*-cells at the beginning of the cycle, these connections being of such a kind that in future the activity of the *E*-cells inhibits that of the motor systems.

The exact way in which we shall visualize the second process depends on our view of (*a*) the nature of inhibition, and (*b*) the nature of the formation of new channels in general. If we take MacDougall's view that both these processes are due to 'drainage,' then it seems natural to suppose that in this case the new channel *AE′* is such that by it energy can be drained off from *A* when *E′* is active, but that *E′* cannot discharge along it to *A*; *i.e.* that it is a new channel in the onward direction only. Then on the second occasion when the animal is placed in the Yerkes box, *E′* is excited through the (onward) channel *SE′*, and inhibits *A* through the (onward) channel *AE′*. On this view (which is essentially the same as that of James and Lloyd Morgan) the chief difference between positive and negative learning lies in the fact that in negative learning only onward connections are formed, while in the positive case connections are also formed in the reverse direction. Another possible theory, which explains the fact of negative learning equally well, is that the connection formed between E' and A consists of specifically inhibitory fibres, *i.e.* that it is a 'reverse inhibitory connection.' Which of these theories is true is a physiological problem on which experiments on animal behaviour do not seem likely to throw much light. On either theory we should expect the observed fact that chickens, for instance, on seeing a nasty object for the second time, often wipe their beaks without pecking at the object —owing to the existence of an onward connection such as SE' in Fig. 6.

It is not certain whether all E-cells can be supposed to have this property of forming inhibitory connections with motor systems whose activity precedes the excitement of the E-cells at the beginning of the cycle, or whether this is a special property of the E-cells in such cycles as those of escape, rejection, etc. The way in which a dog, for instance, learns to avert his gaze from food which is out of reach or from his own image in a mirror suggests that other E-cells may share this property. It seems probable that a study of the form of the learning curves in a suitably devised experiment might throw light on this question, and show whether the neural processes involved in learning to avoid painful stimuli and learning to avoid useless actions (such as trying to get food from a closed glass vessel) are similar in nature, or whether the E-cells in such cycles as that of pain have a special property not shared by the E-cells of such cycles as that of hunger.

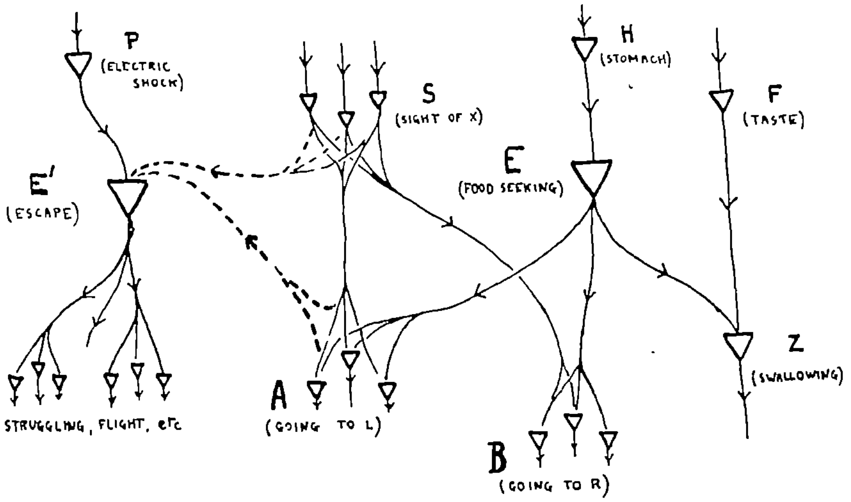

Fig. 6.

Broken lines indicate channels which are formed or strengthened during learning.

XI. SOME DIFFICULTIES OF THE THEORY.

It has been shown in this paper that if we suppose that there are groups of "*E*-cells" which are connected innately, or which become connected by association to various sensory and motor systems in ways which are discussed, we may explain a number of the observed facts about 'behaviour cycles' and 'learning by experience.' But no attempt is made in this paper to describe the way in which such sensory and motor systems work, and it must be admitted that there are grave difficulties in the way of any such attempt.

Consider, for instance, the sensory system which must be supposed to underlie the perception of an object, such as a table. We can form no satisfactory picture of how there can be one central system of neurones stimulated by such widely different retinal patterns as those due to a table seen at various angles and distances, and yet not stimulated by other objects of similar colour. Similar difficulties are met with when we try to explain physiologically how we can recognize a tune heard in different keys, or to explain the behaviour of Kohler's hen. These are, of course, some of the problems from which the '*Gestalt*' psychology starts. If we assume that there *is* a physiological interpretation, we are almost forced to conclude that it involves some new neural principle which has not yet been discovered by the physiologists.

The difficulties in regard to motor systems appear at first sight to be less formidable; since it may be supposed that in many cases, such as that of walking, the motor system is similar in principle to a very complex chain reflex system. But there are some experiments—such as those of Michotte—which show that when a subject has learnt to perform a given series of movements he can without further learning perform a similar series on a smaller scale; and such experiments suggest that the operation of motor systems may involve the same undiscovered neural principle as does that of sensory systems.

XII. FURTHER EXPERIMENTS SUGGESTED BY THE THEORY.

The main assumptions of the *E*-cell theory can only be tested directly by physiological experiment. One possible line of such research is suggested by the fact that an animal deprived of all its *E*-cells would, on the theory, still be able to form habits by 'conditioning,' but would not vary its behaviour in pursuit of ends or learn by experience.

But when we try to develop any one part of the theory in detail, in particular when we try to express any of the supposed physiological processes in a quantitative way, we always find a number of questions arising which can be tested by experimental observation of the behaviour of intact animals. (This is, perhaps, the chief value of physiological theories in psychology.)

One point which arises is that if the theory of learning given above is true, 'learning curves' do not indicate any one physiological process but the resultant of several processes. The curve of learning of an animal represents not the rate at which any one channel is strengthened, but the effect of the strengthening of three channels such as *SE*, *SB*, and *EB* (Fig. 5), the last of these being strengthened by 'reverse association' as well as by 'neural habit.' It would be of great interest to devise experiments which would separate the effects of these processes. This ought to be possible since the excitement of a sensory system depends chiefly on the presence or absence of the appropriate stimuli, and that of the *E*-cells of food-seeking chiefly on the degree of hunger of the animal, and since it is possible to arrange that all the sensory systems in a behaviour cycle shall not be connected to all the motor systems (except through the *E*-cells). Excitement of the *E*-cells would show itself in a tendency to perform any of the actions of the cycle, while excitement of a sensory system would show itself in a tendency to perform especially those actions whose motor systems were connected directly to the sensory system.

Another important question is that of the relation of the time which elapses between the performance of an action and the attainment of the end of the cycle and the rapidity of learning to perform that action. A study of the effect on this of other actions and stimuli intervening between the action studied and the attainment of the end might throw considerable light on the mechanism of the formation of neural associations.

Other lines of research connected with the theory have been suggested earlier in the paper (Sections V, VI, IX and X).

XIII. POSSIBLE EXTENSIONS OF THE THEORY.

It will only be possible to point out very briefly certain directions in which the *E*-cell theory, if it is found to be satisfactory for the simple phenomena discussed in this paper, may usefully be extended to more complex processes.

(1) *Thinking out a Problem.* The kind of learning which we have discussed involves only the reinforcement or inhibition of the tendency to an action which the animal has already performed in a certain situation. There are, however, some cases in which an animal or a man faced with

a problem thinks out a way of solving it even though it has never before solved a similar problem in that way. There are many good instances of this in the behaviour of the apes studied by Kohler. Now the suggested solution which occurs to the animal is always something which is associated with the present situation and with the end to be attained, and we usually find that the association between the situation and the suggested solution is 'forwards,' while that between the end and the suggested solution is 'backwards.' If we compare this with the account given above (Sections V and IX) of the double innervation of the motor systems in positive learning, we see that the two cases are in this respect similar. But when the animal thinks out the solution before acting, it would seem that the neural systems underlying the thought of a line of action do not in this case discharge at once into the muscles. In some way they go on rousing relevant associations (as the animal elaborates the suggestion in its mind) until their activity reinforces that of the *E*-cells (when the animal sees that that line of action is likely to lead to the attainment of the end), and it is only then that the *E*-cells discharge into the motor systems concerned. This account of the neural processes in thinking raises a number of neural problems and is, of course, hopelessly inadequate. All that I would point out is that thinking, like simple learning by experience, seems to involve multiple innervation, one of the connections concerned being a reverse connection with a group of *E*-cells.

(2) *Derived Ends.* The fact that a great many ends are 'secondary' or 'derived,' *i.e.* that men and animals vary their behaviour in pursuit of situations which are not innate ends, seems to imply that under some conditions systems of cells which become connected to *E*-cells can to a certain extent take on the properties of *E*-cells. There is need for a detailed study of this process, but I do not think that it necessarily implies any new neural principle.

(3) *Abnormal Psychology.* Reference has already been made to MacDougall's theory of the neural basis of repression, etc. (Section I). It is perhaps not too much to hope that further work along the lines suggested by MacDougall will clear up a number of the outstanding problems about the working of the 'unconscious mind.'

XIV. SUMMARY.

The theory discussed in this paper, and here called the '*E*-cell' theory, is that underlying the tendency to pursue any end there is the activity of a powerful group of neurones, which cease to be active when the end is attained. This had been suggested by James, Woodworth, and MacDougall (Section I).

It is shown that this theory can be made to explain in a simple way the variation from one kind of behaviour to another which is a characteristic mark of purposive activity (Section V). It is suggested that the reasons for the quiescence of the *E*-cells when the end is attained are probably different in different behaviour cycles (Section VI).

The theory leads to a view of the neural processes in learning which is essentially similar to that of Lloyd Morgan. It is supposed that *E*-cells have the property of forming reverse excitatory connections with motor systems whose activity precedes the attainment of the end of the cycle (Section IX), and that some *E*-cells, if not all, have the property of forming reverse inhibitory connections with motor systems whose activity precedes the excitement of the *E*-cells at the beginning of the behaviour cycle (Section X). The theory, in this form, explains a number of the observed facts of learning (Sections IX and X).

The theory supposes that the motor systems involved in any purposive action are usually stimulated partly by sensory systems and partly by *E*-cells (Section V). It is suggested that the same kind of double, or multiple, innervation is involved in thinking out the solution of a problem (Section XIII).

Certain difficulties in connection with the nature of sensory and motor systems are briefly discussed (Section XI), and also certain possible extensions of the theory (Section XIII), and some possible lines of experimental research suggested by the theory (Section XII).

I wish to express my thanks to several friends with whom I have discussed portions of this paper, especially to Professors Chattock, Lloyd Morgan, and Field.

(*Manuscript received* 15 *July*, 1927.)

TRANSFER EXPERIMENTS WITH GUINEA-PIGS

BY C. V. D. HADLEY.

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(1) GENERAL NATURE OF THE EXPERIMENTS.

Transfer experiments are attempts to specify and limit the kinds of changes which can be made in some situation without destroying the habitual reaction which the animal has acquired with regard to that situation.

Adjustment to an otherwise familiar situation in which change has occurred can, of course, take place by means of different mechanisms of mental activity. Adjustment can be conditioned by imaginal recall of the familiar situation, or some part or feature of it, which, together with the new situation, determine behaviour.

As an illustration of the sort of thing meant an example from human behaviour can be chosen. Suppose that a man enters a darkened room with the object of getting something that he has left on the mantlepiece. The room is supposed to be quite familiar and to have suffered no other change except that now it is dark, while all former activities in it have been in daylight or in artificial light. It is extremely probable that some explicit recall of the features of the room and the mantlepiece will be utilized to aid the man in his search. Especially if he does not remember exactly where on the mantlepiece the desired object is situated, a visual image of the mantlepiece, with the disposition of the other objects on it, will probably guide him as to the likely places between these objects in which to feel without risk of knocking the other things over.

Again adjustment can be attained by "thinking about the situation" when mere imaginal recall is insufficient, that is, by the utilization of free ideas. To chose a simple example, suppose that a man's habitual mode of dealing with a door knob does not produce the desired result. In this case the mere ideal recall of what the door knob looked like or did on the last occasion on which it was successfully manipulated would be of little help. The man may therefore reflect on the vagaries of door knobs in general, and make an attempt to deal with the door knob in light of one or other of the several possible misdemeanours of which door knobs are capable, as suggested by his general knowledge of such objects.

Now since it is unlikely that most animals can utilize the last method, and since—though it is extremely probable that the power of imaginal recall is possessed by some animals—the occurrence of 'images' in animal life seems to be restricted; then, if an animal successfully transfers its habitual mode of dealing with a particular situation to another similar situation but differing in certain respects, unless there is clear evidence for suspecting the occurrence of images, some other mechanism must be at work than those either of imaginal recall or of thinking. This is the mechanism of transference; and transfer experiments seek to elucidate the nature of the processes at work and to specify the kinds of changes which can be successfully overcome by these processes.

(2) GENERAL NATURE OF THE SITUATIONS.

The situations dealt with in my experiments were as follows:

In each situation as a whole there were two illuminated (electric light transmitted through opal glass) square areas against a dark background. One square was always larger than the other, thus introducing, besides a difference in size, a difference in the intensity of light in the two bays which contained the squares (see description of apparatus), the bay containing the larger square being more brightly illuminated than the other. The situation as a whole contained, therefore, two components or 'stimuli' which differed from each other both in size and in intensity of illumination.

The animals were trained either (*a*) to choose the more brightly illuminated bay, that is, the bay containing the larger square, or (*b*) to choose the lesser illuminated bay, that is, the bay containing the smaller square.

(3) GENERAL NATURE OF THE TRANSFER EXPERIMENTS.

(*a*) The first transfer experiment consisted (1) in the case of the animals trained as in (*a*) above, in removing the 'negative stimulus'; that is, the smaller square, altogether, but leaving the 'positive stimulus,' that is, the larger square. The old negative stimulus was now replaced by a new square which bore the same sort of (not identical) relation to the old positive stimulus as that bore to the old negative stimulus.

Thus an animal was trained, on a situation containing a 3-inch square *versus* a 1-inch square, to go to the 3-inch square. Then the 1-inch square was removed, and an 8-inch square introduced in its place. The general nature of the two situations remained the same.

Similarly with an animal trained as in (*b*) above, the negative stimulus, this time the larger square, was removed and a new square introduced which varied to the positive stimulus as that varied to the old negative stimulus. For example, with an animal trained, in a situation containing an 8-inch square *versus* a 3-inch square, to go to the 3-inch square, the 8-inch square was removed and a 1-inch square introduced.

(*b*) The second transfer experiment was as follows:

In the situation as a whole there was a difference both in the intensity of illumination in the two bays as well as a difference in size in the two squares. Thus there were, theoretically at any rate, two possible means of making a discrimination, (*a*) either by difference in illumination, or (*b*) by difference in size of the squares.

The experiment consisted in presenting a situation in which one of these possible means of discrimination was eliminated and the other kept. That is, animals trained as above on the 8 *v.* 3 situation, were then transferred to an 8 *v.* 3 situation in which the intensity in the two bays was made equal and only the difference in size of the two illuminated areas remained.

(4) Animals Used.

The animals used were guinea-pigs.

Eleven animals were used in all, eight of which were males. Their ages at the commencement of experimentation with each animal were as follows:

Males 1 and 2 }
Females 1 and 2 } about 1 year
Male 3 about 3 months
Males 4, 5, and 6 about 2 months
Males 7 and 8 }
Female 3 } about 1 month.

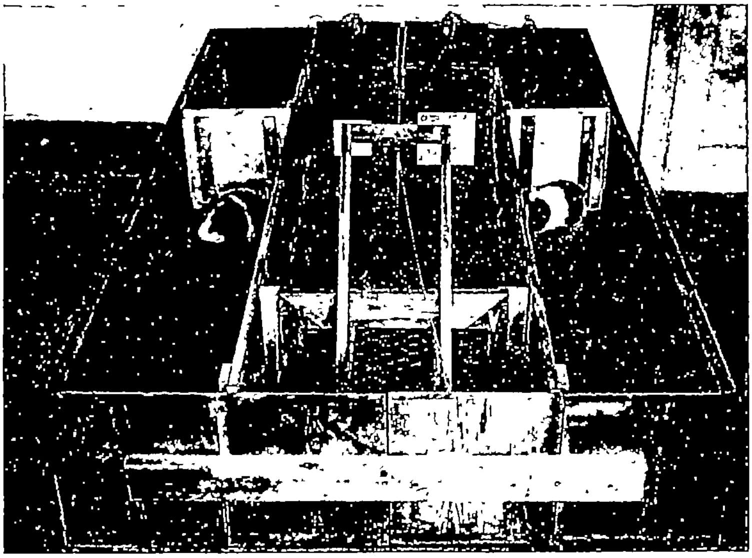
Males 7 and 8 and Female 3 were the offspring of Male 1 and Female 1.

(5) Description of the Apparatus.

The accompanying photograph gives a general view of the apparatus as seen from the experimenter's seat. In the foreground is seen the release box (the wire roof was removed during experiments) with the method of opening the release-box door. This door was made of celluloid and was fastened by means of a catch fitting into a slot in the floor of the runway. The catch was released by pulling the string which at the same time raised the door, thus setting free the animal.

The light boxes are seen at the other end of the apparatus, the heads of the two electric light bulbs (both of the same power) showing on the top of each box. Each lamp was connected with a variable graduated resistance. In the face of each box was cut a square opening just larger than 8 inches square. In each opening was inserted a square of opal glass, the white squares seen in the photograph. In front of each square of opal glass stiff cardboard sheets containing the necessary-sized squares were inserted in grooves placed at each side. This permitted, according to the size of the square, a certain illuminated area of opal glass to face on the runway.

The food boxes are seen on each side of the light boxes. They were separately illuminated by small electric bulbs placed in the far corners nearest the light boxes. The food-box doors are situated in the bottom of the corners made by the face of the light boxes and the side walls of the runway. They opened into the food boxes and were arranged so as to fall back to the shut position after the animal had entered the food boxes. They were easily opened by just pushing on them. When necessary they could be locked from the inside. They were made of stiff celluloid. The partition can just be seen projecting from between the two light boxes, this made two bays in each of which is contained a square and a food-box door.



N.B. The two squares shown in the photograph were never used together in any experiment.

The apparatus as a whole was raised by trestles about a foot above the ground.

The two animals shown in the photograph are respectively Male 1 on the left and Female 1 on the right. The alleyways in which they are placed were not used during experiments.

The experimenter sat directly behind the release box and worked the release-box door by means of the string and pulley.

The inside measurements of the apparatus were as follows:

			ft.	ins.
Runway	1.	Release box to squares	3	6
	2.	Width	1	7½
	3.	Depth	0	11
Partition	1.	Projection into runway	0	7½
Release box	1.	Length	0	11¾
	2.	Width	0	5½
	3.	Depth	0	8¼
Food boxes	1.	Length	2	0
	2.	Width	0	10½
	3.	Depth	0	11
Food box doors	1.	Length	0	5¾
	2.	Height...	0	4
Bays	1.	Width	0	9¾

(6) EXPERIMENTAL PROCEDURE.

(a) *General.*

An animal was taken into the experimental room and placed in a small box which stood on the floor near the far end of the apparatus. The release-box door was then locked, the squares adjusted, food placed in the food boxes, and the food-box doors arranged: the door in the same bay as the negative square being locked, the other door closed but not locked. Regarded from the runway there was no perceptible difference between these doors. The animal was then taken from the floor box and placed in the release box, and when properly oriented was set free by raising the door.

When the animal had made its choice and entered the food box it was left until it had eaten the food—a small piece of carrot usually—and then taken out and put into the floor box, and the procedure repeated. Reward alone was used, no punishment being inflicted. The experiments took place in the evenings from 5.30 onwards.

(b) *Feeding.*

Each animal was fed immediately after being experimented with, the food being bran and oats, grass, and hay. Carrot was used as incentive or reward.

The remains of the meal were removed the next day at 10 a.m., the animals being thus left without food, except for a certain amount of hay, until they were again fed after experimentation the same night.

(c) *Series of Trials.*

Each animal was, as a rule, given a series of ten trials at each time of experimentation. Sometimes owing to the behaviour of the animal (fright, etc.) or for other reasons it was given less, but never more than ten. The series were arranged so that the positive square came in the left bay five times and in the right bay five times in an irregular order. Such a series was called a *regular* series. The order also varied for each animal; so that one animal never had an identical training series as another animal. The chief reason for this was due to the method adopted to break up the position habits which the animals fell into. This consisted in locking the door to which the animal habitually went—the negative square being of course kept in the same bay—for the majority of trials in one or several series, or throughout several series. When the animal showed signs of giving up the habit, the *regular* series was recommenced. This, of course, introduced irregularity, as the position habits of the various animals differed both in regard to direction and duration.

(d) *Correct and Incorrect Choice.*

An animal was considered to have made a correct choice if it nosed open the correct door and entered the food box without having nosed the incorrect door, or without having entered bodily into the incorrect bay. If the animal approached and paused at the entrance to the incorrect bay and then nosed the correct door and entered the food box, this was counted as a correct choice.

(7) BEHAVIOUR DURING LEARNING.

The behaviour of the animals during the learning period can be divided into three phases, which, while not sharply marked off from each other, can be distinguished by the general type of behaviour which occurred in each phase. I shall name them respectively (1) the Emotional Phase, (2) the Phase of Directed Effort, and (3) the Phase of Perfecting the Correct Response.

It should be said that none of the animals was given a prolonged Acquaintance Series of trials. Before beginning experimentation proper each animal was given just sufficient time in the discrimination box to enable it to learn the method of opening the food-box doors, but not long enough to become thoroughly familiar with the apparatus. When an animal had learned how to push open the food-box doors and enter the food boxes, training proper was commenced.

From what will be said later it will be seen that the phase of learning which has been called the Emotional Phase corresponds more or less to the usual Acquaintance Series of trials.

(*a*) *Emotional Phase.*

The first phase has been called the Emotional Phase on analogy with human experience, as it seems to correspond to that period of emotional and effective adjustment which the average person undergoes in the face of any new situation.

During the Emotional Phase the guinea-pig exhibits at least five different kinds of behaviour. By different kinds of behaviour I mean behaviour which is initiated and sustained by different tendencies or conative dispositions, and not that the behaviour, externally regarded, is capable of being distinguished into five classes. The classification therefore is in a measure hypothetical.

For example, 'gnawing' might be listed as a type of behaviour which occurs in this phase, but the persistent gnawing of a closed food-box door and the casual gnawing of the partition *e.g.*, while both are examples of 'gnawing,' would appear—especially when the totality of the behaviour on each occasion is taken into consideration—to differ, in that they are initiated by different tendencies or conative dispositions in the animal.

The different tendencies which appear to be specifically at work in this phase of learning are:

(1) Curiosity or Exploration.
(2) Fear with Timidity.
(3) Pugnacity.
(4) Food-seeking.
(5) Play or 'excitement.'

One guinea-pig may exhibit all the different kinds of behaviour in the course of one trial, or one kind of behaviour might occur predominantly, *e.g.*, in the earlier trials of a series, and another kind in the later. The following description relates especially to the behaviour of the younger animals—Males 4, 5, 6, 7, and 8, and Female 3.

(1) *Curiosity or Exploration.* The behaviour which seems ascribable to a curiosity or exploration tendency takes the form of running about and poking in various corners, nuzzling or nosing, gnawing at any protruding object, and climbing up, that is, standing on the hind legs and placing the fore feet on some supporting object. They will climb up in the release box (especially), on the partition, on the face of the squares, in the food boxes, in fact anywhere, and frequently stay for fairly long times in this position. All the behaviour which occurs during this phase is frequently accompanied by squealing and one animal—Male 7—had a peculiarly modified way of squealing when he was performing this curiosity behaviour which seemed specially related to such behaviour.

(2) *Fear with Timidity.* Fear gives rise to a quick dash into any neighbouring corner or back into the release box; the animal then stops and remains quiet. This dash is frequently accompanied by a low 'gurr.' This 'gurr' usually accompanies the perception of any sudden stimulus which sharply attracts the animal's attention, any sudden noise or movement, for example. The animal 'gurrs,' stops whatever it is doing, perhaps faces the direction of the sound, if it is not too far away, and remains motionless. If the stimulus is not repeated and if the animal is not particularly timid, it will, after a while, resume activity.

Timidity. It seems necessary to distinguish timidity from fear. Fear produces a temporary reaction which in a non-timid animal is more or less quickly recovered from. On the other hand, should a timid animal be frightened it will in all probability refuse to work any more for that evening. A timid animal shows a great disinclination to move about at all and does so gingerly and hesitatingly. If it is frightened it relapses into an immobility response and refuses to budge. If pushed it either resists or allows itself to be passively pushed along, and then squats down again. Frequently it turns round and runs from the direction in which it is being pushed.

Timidity would appear to be a general temperamental trait, while the fear reaction is a specific reaction to more or less specific stimuli.

Timidity would, then, be a name for a temperamental tendency or disposition which qualifies all the specific reactions of the animal possessing it, while not leading to a specific mode of response itself. Thus it qualifies the usual fear reaction and turns it into an immobility response with cessation of work, and also renders the fear reaction much more sensitive, that is, lowers its threshold; it turns the usual free-running-about of the animal into hesitant movement with jerks and stops, etc.

Timidity therefore has a general effect on the whole behaviour of the animal, and this seems to be the characteristic effect of what are ordinarily called temperamental traits, tendencies or dispositions.

Close observation and much patience will be almost certain to reveal other temperamental tendencies among such animals as guinea-pigs. It will be a difficult task, however, as, though it is perfectly obvious to any onlooker that one animal's behaviour taken as a whole differs from that of another, it is difficult to say, except in the most general terms, in what the difference essentially consists. And if in any special case a list of such differences should be made out, then, assuming the above account of the mechanism of a temperamental tendency to be correct, a further difficulty arises: are these differences of such a nature that they would appear to be conditioned by a single tendency qualifying the expressions of the other specific tendencies, or are they merely heterogeneous differences due to differences in the constitution of the specific tendencies themselves? This is a very general theoretical point, which might be illustrated in the following way. When we say that *A* is, *e.g.*, a retiring and modest person, we mean that in all, or in a great many of *A*'s activities and relations to the world in general we can observe a certain something in which they all resemble each other. This something which pervades all the behaviour of *A* we call 'modesty' and ascribe it to an innate tendency or disposition in *A*, working in the way suggested above. The possession of 'modesty' by *A* will serve to differentiate *A* from quite a number of other people.

But again, *A* might possess a bundle of characteristics which might serve to differentiate him from *B*, but which do not possess among themselves any common similarity, in the same way as those other characteristics possessed or exhibited the common similarity of 'modesty.' It is suggested that only in the first case should we call the differentiating factor a temperamental tendency.

On account of excessive timidity Female 2 had to be discarded. Male 8 also suffered from this defect which in combination with a persistent position-habit rendered him useless for experimental purposes[1].

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[1] It seems a curious thing that these animals are, on the whole, quite unperturbed by a loud peal of thunder, and I have had even a relatively timid animal work quite well with a class in a room above the experimental room making a considerable din. The non-timid animals do not mind at all. Whether the fact that thunder and the noise upstairs did not upset them is due to the direction of the noises, *i.e.* from above, whereas the noises that they usually react to come from the ground-level, or from some other cause, I am unable to say. It might be that guinea-pigs in their native habit have nothing to fear from the air, but everything from the ground. In this connection it might be mentioned that the rustling of hay disturbs them immediately.

(3) *Pugnacity.* The only examples of behaviour which might be ascribed to pugnacity that I can produce relate to the behaviour of certain of the animals with the food-box doors. One amusing example was produced by Male 2. He is a large boisterous animal. When the discrimination box was newly made it happened that the door of the right food box used to stick a little, and thus be difficult to open. I had noticed that this animal was getting into the habit of dealing very vigorously with this door, he would give it an extra hard push, whereas he dealt more leniently with the left food-box door which swung easily. When I discovered what was wrong I had the door trued up to swing easily. The first time the animal encountered the door after this had been done he as usual gave it an extra hard bang with a sideways motion of the head and proceeded to enter; what happened was that the door rebounded vigorously and hit him on the nose as he was entering, whereupon he hit another hard bang with the same result. He thereupon set to and had a fight with the door, and for trial after trial for a considerable time this animal would regularly fight this door, although it now swung as easily as the left door which he did not fight at all.

Similarly, Male 4 at one time fell into a position-habit for the right food-box door, that is, for every trial for a considerable period he would go straight out to this door and attempt to open it: if it was locked he would spend some time gnawing at it, scratching near it, nosing it vigorously, and in the course of one trial he might make several periodic attacks of this nature before finally leaving it alone and entering the other food box.

(4) *Food-seeking.* Definite food-seeking behaviour is not very easily, if at all, separable from curiosity behaviour, but especially towards the end of the Emotional Phase the behaviour becomes much more directed to the relevant parts of the apparatus, *e.g.* the food-box doors, and there is less indiscriminate running about and climbing up; seemingly one particular tendency is becoming predominantly directive in the behaviour of the animal. That is, all endeavour seems now to be directed to getting into the food boxes and to the food, and no concern is shown for exploration of other parts of the apparatus.

(5) *Play.* This peculiar kind of behaviour is called 'play' for want of a better name.

In the first place I have only observed it in the quite young animals, Males 4, 5 and 6. Their ages are not known exactly, but they were probably under two months when first put into the apparatus. I have not noticed it in any of the older animals.

The actual behaviour consists in a quick running up and down for a few times a certain length of the runway, the end of each run being marked by a quick check and hop-around. That is, the animal will suddenly run a little way down the runway, check quickly, hop-around, run back up the runway, check quickly and hop-around, and repeat this several times. The hopping-around is the peculiar part; the animal does not merely turn round, but hops or jumps around. This behaviour is almost always accompanied by high excited squeaks. I have also seen the animals do it in their cages.

During this phase these different kinds of behaviour succeed each other in a quite haphazard manner, until the more uniform behaviour of the Phase of Directed Effort arrives.

Looking at the matter from the point of view of the tendencies which are supposed to be operating, the transition from the Emotional Phase to the Phase of Directed Effort occurs by the suppression or satisfaction of all the tendencies which are at work in the Emotional Phase with the exception of the food-seeking tendency, which during the Phase of Directed Effort is the dominant tendency.

Continued acquaintance with the apparatus will soon render it so familiar that there will be less and less incentive to curiosity behaviour. Slight timidity will be overcome. The reaction to 'frightening stimuli' will become more discriminative. Play behaviour drops out. Thus, since the general conditions of the experiment and the nature of the food-seeking tendency itself render the latter impossible of satisfaction in the same way as, for example, the curiosity tendency, the food-seeking tendency comes to dominate more and more and assume a directive rôle. The transition from the Emotional Phase to the Phase of Directed Effort is thus gradual.

(*b*) *Phase of Directed Effort.*

This phase is marked by the discontinuance or only the sporadic recurrence, of the indiscriminate exploratory behaviour which characterizes the Emotional Phase. The animal is not so likely to be distracted by accidental noises, etc.; and the behaviour is more directed and continuous, more of a piece so to speak. Again, orientation to the door of the release box becomes precise and regular, a locked food-box door is not returned to several times and tried, nor is an unlocked food-box door nosed open and then left to fall back, but the food box is entered immediately.

Position-Habits. It is necessary to discuss the relation of position-habits both to this phase and to the Emotional Phase. Position-habits occur all through the learning period; only in the Emotional Phase they are masked by the activity characteristic of that phase, whereas in the Phase of Directed Effort they become much more definite. For example, Male 7 learnt his discrimination 8 *v.* 3 to choose 3 in 125 trials, that is, he took 125 trials before making a series of 30 correct trials. These 125 trials were spread over approximately three weeks—January 16–February 5, 1925. This is quick learning for a guinea-pig. Making a more or less arbitrary division we can say that the first 53 trials are included under the Emotional Phase described above. In these the positive square, *i.e.* the 3-inch square, was on the left 27 times and on the right 26 times. The animal's first choices were as follows: right door first 21 times, left door first 32 times, that is, taking the whole of the 53 trials, the first choices were fairly evenly distributed, with, however, a slight partiality for the left-hand door.

In only 19 cases did he make a correct choice.

Now in the 24 trials succeeding these 53 he chose the right door first 22 times and the left door first only twice. This was the Phase of Directed Effort.

It so happened that in the case of this animal his taking up of a definite position-habit—quite a brief one—coincided with the occurrence of the Phase of Directed Effort, while his Emotional Phase was free from the position-habit factor. But this is not usual, as position-habits frequently occur in the Emotional Phase.

For example, Male 4 learnt his discrimination 3 *v.* ½ to choose 3 in 206 trials—November 9–December 5, 1924. In the first 116 trials—positive square on the right 65 times, on the left 51 times—he chose the left door first 79 times and the right 37 times, thus showing a distinct partiality for the left food-box door. In the 90 succeeding trials—positive square on the right 25 times, on the left 65—he chose the right door first 80 times and the left first only 10 times. That is, he changed his position-habit. The occurrence of the Phase of Directed Effort bears no relation to the above, as he only settled down to directed effort shortly before learning the discrimination correctly. (This is the animal which displayed 'pugnacity' in relation to the right food-box door.) His Emotional Phase was therefore quite prolonged.

To take another example, Male 5 learnt his discrimination 8 *v.* 3 to choose 3 in 141 trials—November 9–30, 1924. The Emotional Phase lasted for about 80 trials—positive square on the right 36 times, on the left 44 times. He chose the right door first 53 times and the left door first

27 times, thus showing a partiality for the right food-box door. In the 61 succeeding trials, which include the Phase of Directed Effort as well as the Phase of Perfecting the Correct Response—positive square on the right 28 times, on the left 33 times—he chose the right door first 32 times and the left door first 29 times.

Thus this animal, while displaying fairly distinct Emotional and Directed Effort Phases, did not display any well-marked position-habit, which, such as it was, occurred in the Emotional Phase.

Position-habits are sometimes difficult to deal with, in some cases they can be broken up fairly easily. The chief method employed was to lock the door—of course keeping the negative square on the same side—to which the animal went persistently in all the trials or in the majority of trials throughout a series or several series, until the animal gave up the habit and then to revert to the *regular* series.

This had various effects. In the majority of cases it did succeed in breaking up the habit. But in the case of Male 4 quoted above, the locking of the right food-box door during the period in which he developed the right position-habit only increased the energy and persistence with which he attacked it, and as soon as this method was abandoned he began to pick up the discrimination.

Again, Male 8, a timid animal, had been given 250 trials through practically the whole of which he maintained a consistent left-hand position-habit on which locking the door through a large number of trials had no effect.

Other Examples of Position-Habits. Male 7 displayed one curious position-habit. At one time he fell into the habit of always going out of the release box oriented in the direction of the left bay. After some time the slight noise that the release-box door made on opening attracted his attention; this at first caused him to pause just outside the release-box door for a bit and then continue straight on. After a bit he began to turn back to the release box; that is, he would go out, check, turn back, pause a bit, then turn round again and continue down the runway. He turned back to the release box leftwards, and, continuing the same motion round again, this oriented him in the direction of the right bay, the consequence was that he started to fall into a right-hand position-habit. This was not the end, however; he developed the habit further. The pulley of the release-box door was oiled as soon as the cause of his behaviour became apparent; this minimized the noise but did not get rid of it entirely. The final form of the habit was as follows. Instead of checking and then turning back, the animal would go out, turn a complete circle leftwards just outside the release box and then run down to the right bay. This soon became entirely habitual.

Male 3 developed the habit of always running down the right runway and checking some little distance outside the right bay before making his choice. This seemed to have been caused initially by fear of me—he would scurry out of the release box down the right runway and then pause (typical fear reaction). Even when fear of me was overcome, the habit was worked into his general behaviour. If a habit can be 'worked into' the total 'Correct Response Behaviour' it is done preferably to dropping it out or modifying it even though to drop it out or modify it would make for an easier—*i.e.* from the point of view of the 'logical' human observer —Correct Response.

(c) *Phase of Perfecting the Correct Response.*

The Phase of Directed Effort passes gradually into the learned habit through what I have called the Phase of Perfecting the Correct Response. Of course correct responses occur in the Emotional Phase and in the Directed Effort Phase; but in such cases they are not due to discrimination, but to chance or position habit, that is, they are not the result of discriminatory behaviour. The Phase of Perfecting the Correct Response is essentially the phase of perfecting discrimination or discriminatory behaviour. Once again it is difficult to say when exactly discrimination begins. The most general feature of behaviour which is truly discriminative is the correction of wrong orientation. If an animal goes out of the release box oriented in the direction of the negative stimulus, and then corrects its orientation in the runway, and goes to the positive stimulus, and does this a number of times in one series, this is good evidence that discrimination has begun. The occurrence of such behaviour characterizes the Phase of Perfecting the Correct Response. In one case this phase appeared abruptly (Male 7). I described above his Emotional Phase and Directed Effort Phase. Immediately following on the last series—a series was 8 trials in this case—of the latter phase he gave a wholly correct series of 8 trials.

This was not just chance, as several times he was oriented wrongly when he left the release box and corrected himself while in the runway.

Unfortunately the animal did not continue his initial success or there would have been no Perfecting of the Correct Response Phase in his case. He required 40 more trials (4 series of 10 each), that is, 48 trials in all, before giving a sequence of 30 correct trials.

In no case has there been a sudden appearance and continuance of the correct response.

Female 1 supplies an illustration of discriminatory behaviour which did not occur with any of the other animals. This behaviour was as follows: she would pause either near the end of the partition or a little way in front of either bay, and then turn to each bay alternately several times, but without approaching either bay. For example, she might first of all pause and face the left bay, then turn and face the right bay, then perhaps face back to the left bay, etc. After doing this for a few times she would make her choice by approaching one bay and nosing the door. Of course at first she was as often wrong as right. As discrimination grew more perfect this 'comparison' behaviour gave way to what one might call 'direct recognition.' If she approached the wrong bay, then a slight glance and pause was enough, and she would immediately approach the other bay and nose the door. There was no protracted 'comparison' at this stage.

It might be mentioned that this animal was the gentlest as well as the most unhurried of all the animals, and it might be that this 'comparison' behaviour was related to her peculiar temperament, while the more vigorous animals had so to speak no time for such behaviour. Discrimination having once appeared it is gradually perfected throughout this phase until it passes into the behaviour of the Learned Habit.

(8) LEARNED HABIT.

When an animal has learned the discrimination perfectly it maintains a customary direction in running down the runway; for example, if discrimination grows out of a right-hand position habit the animal will first approach the right bay and then swerve to the left if the correct square is there, or continue on into the right bay and right food box if the correct square happens to be there. Some animals run down the middle of the runway and swerve to right or left as the case may be when near the partition.

Male 1 made his discrimination directly on going out of the release box, that is, he ran straight to the right or left bay as the case might be. He was the only animal to do this consistently. The other animals always made their discrimination further down the runway, that is, they only gave up as much of their former behaviour as was consistent with the making of the correct response. In other words, their customary direction of going out of the release box, and part of their position-habit, was continued into the learned habit phase. If this habitual way of coming out of the release box and running down the runway is lost or interfered with, discrimination is almost sure to suffer, the animal begins again to make mistakes. In the early part of the experiments a sequence of twenty trials all correct was taken as proof that the animals had learned the discrimination; later on, a sequence of thirty trials or one mistake in thirty was demanded.

(9) RESULTS OF THE TRANSFER EXPERIMENTS.

(a) *First Transfer.*

Theoretically considered, the result of the first transfer experiment might have been one of three possibilities:

(1) The animal might break down completely.

(2) The animal might go to the old 'stimulus' to which it had been accustomed.

(3) The animal might go to the new 'stimulus.'

The third possibility was the one which occurred predominantly. That is, the animals now treated the old 'positive stimulus' as the 'negative stimulus,' and went to the new 'stimulus' which bore the same sort of relation to the old 'positive stimulus' as that bore to the old 'negative stimulus.'

The animals were not, as a rule, given more than 30 or 40 trials in the early transfer experiments[1], since with increasing number of trials it would become more and more difficult to separate the effects of transfer as such from training.

1. Animals trained on 3 *v.* ½ to go to 3, then transferred to 8 *v.* 3.

(*a*) *Male* 1. Required 127 trials before making a sequence of 20 trials all correct on 3 *v.* ½. (April 25–May 11, 1924.)

Transferred to 8 v. 3 (*both food-box doors unlocked*).

Series 1. (*May* 12.)

Trials	L.	R.	1st choices	
1	3	8	3	L. w.
2	8	3	8	L. c.
3	3	8	8	R. c.
4	8	3	8	L. c.
5	3	8	8	R. c.
6	3	8	8	R. c.
7	8	3	8	L. c.
8	3	8	8	R. c.
9	8	3	8	L. c.
10	8	3	8	L. c.

Series 2. (*May* 13.)

Trials	L.	R.	1st choices	
1	8	3	8	L. c.
2	3	8	8	R. c.
3	3	8	8	R. c.
4	8	3	8	L. c.
5	3	½	3	L. c.
6	3	8	3	L. w.
7	3	½	3	L. c.
8	½	3	3	R. c.
9	8	3	8	L. c.
10	½	3	3	R. c.

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[1] Those with both food-box doors open.

Summary and notes. In 20 trials—Series 1, 8 *v.* 3; Series 2, mixed 8 *v.* 3 and 3 *v.* ½—Male 1 chose the 8-inch square 18 times. The 2 mistakes he made were peculiar in that his first choices in both cases were quite correct, that is, he ran out to the bay which contained the 8-inch square, nosed open the food-box door, let it fall, paused a bit, then went and entered the other bay and food box.

1. Male 1 had already learnt the 3 *v.* ½ discrimination in a discrimination box which was discarded in favour of the one described above, thus the 127 trials which he took in the new apparatus were greatly determined by 'memory.'

2. The reason why this animal was not given more than 20 transfer trials was because an attempt was made immediately—too immediately—to break up this new discrimination of going to the 8-inch square, and an attempt was made to get him to go to the 3-inch square in the 8 *v.* 3 situation, the idea being—provided he did learn it—to transfer him back to the old situation. But the result was that the animal grew greatly discouraged and lackadaisical when he found that he was now compelled to turn away from the 8-inch square, and after 240 trials work was given up with him as his behaviour became too 'morose' and no signs of the required discrimination appeared. He did give up going constantly to the 8-inch square after about 30 trials, but then fell into position-habits.

3. At this early stage both food-box doors were unlocked during the transfer experiments so that the animals could enter either food box, *i.e.* go either to the 8-inch square or the 3-inch square and enter the respective food boxes. Food was placed in both.

(*b*) *Male* 2. Required 305 trials before making a sequence of 20 trials all correct on 3 *v.* ½. (April 23–June 2, 1924.)

Transferred to 8 v. 3 (*both food-box doors unlocked*).

Series 1. (*June* 3.)

Trials	L.	R.	1st choices	
1	8	3	3	R. w.
2	3	8	8	R. c.
3	8	3	3	R. w.
4	3	8	8	R. c.
5	3	8	8	R. c.
6	8	3	3	R. w.
7	3	8	8	R. c.
8	8	3	3	R. w.
9	8	3	3	R. w.
10	3	8	8	R. c.

Put back to 3 v. ½ (*wrong door locked as in training,* i.e. ½).

Series 2. (*June* 4.)

Trials	L.	R.	1st choices	
1	3	½	½	R. w.
2	3	½	½	R. w.
3	½	3	3	R. c.
4	3	½	½	R. w.
5	3	½	½	R. w.
6	3	½	3	L. c.
7	3	½	½	R. w.
8	½	3	3	R. c.
9	3	½	3	L. c.
10	3	½	3	L. c.

Series 3. (*June* 5.)

Trials	L.	R.	1st choices	
1	3	½	3	L. c.
2	½	3	3	R. c.
3	3	½	3	L. c.
4	½	3	3	R. c.
5	½	3	3	R. c.
6	3	½	3	L. c.
7	3	½	3	L. c.
8	½	3	3	R. c.
9	3	½	3	L. c.
10	½	3	3	R. c.

Transferred back to 8 v. 3 (*both food-box doors unlocked*).

Series 4. (*June* 6.)

Trials	L.	R.	1st choices	
1	3	8	8	R. c.
2	3	8	8	R. c.
3	8	3	3	R. w.
4	3	8	8	R. c.
5	8	3	3	R. w.
6	8	3	3	R. w.
7	8	3	3	R. w.
8	8	3	8	L. c.
9	3	8	8	R. c.
10	8	3	3	R. w.

Series 5. (*June* 7.)

Trials	L.	R.	1st choices	
1	3	8	8	R. c.
2	8	3	3	R. w.
3	8	3	8	L. c.
4	3	8	8	R. c.
5	8	3	8	L. c.
6	3	8	8	R. c.
7	3	8	8	R. c.
8	8	3	8	L. c.
9	8	3	8	L. c.
10	3	8	8	R. c.

Series 6. (*June* 8.)

Trials	L.	R.	1st choices	
1	8	3	8	L. c
2	8	3	8	L. c.
3	3	8	8	R. c.
4	8	3	8	L. c.
5	3	8	8	R. c.
6	3	8	8	R. c.
7	8	3	8	L. c.
8	3	8	8	R. c.
9	3	8	8	R. c.
10	8	3	8	L. c.

Door corresponding to the 3-inch square and ½-inch square (*locked*).

Series 7. (*June* 9.)

Trials	L.	R.	1st choices	
1	8	3	8	L. c.
2	8	3	8	L. c.
3	½	3	3	R. c.
4	½	3	3	R. c.
5	3	½	3	L. c.
6	3	8	8	R. c.
7	3	½	3	L. c.
8	8	3	3	R. w.
9	8	3	8	L. c.
10	½	3	3	R. c.

Summary and notes. As with Male 1, on transfer the food-box doors were both left unlocked, the animal immediately fell into a right-hand position-habit, choosing the right bay and right food box irrespective of which square was there—Series 1. (As a matter of fact he had displayed a tenacious right-hand position-habit during training on the 3 *v.* $\frac{1}{2}$ situation.)

Therefore in Series 2 he was put back on to the 3 *v.* $\frac{1}{2}$ situation, the incorrect door, *i.e.* the $\frac{1}{2}$-inch-square door, being locked as was usual during training. The animal again started off with the right position habit, but did the last 3 trials correctly.

Series 3 was again a training series, which he did quite correctly.

In Series 4 he was transferred once more to the 8 *v.* 3 situation, both doors again being unlocked. He once more fell into a right position-habit, so from the 7th trial of this series and through the remaining series the door corresponding to the 3-inch square was locked.

In Series 5, the door on the same side as the 3-inch square being locked, he made 1 mistake, *i.e.* chose the 3-inch square once.

In Series 6, under the same conditions, he made no mistake.

In Series 7, a mixed series, he made 1 mistake.

This animal therefore failed to transfer when both food-box doors were unlocked, but only required 4 trials with the door corresponding to the 3-inch square locked before making a sequence of 20 trials with 1 mistake, and with 1 mistake in the mixed series of 10 trials.

(*c*) *Female* 1. Required 280 trials before making a sequence of 20 trials all correct on 3 *v.* $\frac{1}{2}$. (April 26–May 29, 1924.)

Transferred to 8 v. 3 (*both food-box doors unlocked*).

Series 1. (*May* 30.)

Trials	L.	R.	1st choices	
1	8	3	8	L. c.
2	3	8	8	R. c
3	8	3	8	L. c
4	3	8	8	R. c.
5	3	8	8	R. c.
6	8	3	8	L. c.
7	8	3	8	L. c.
8	3	8	8	R. c.
9	8	3	8	L. c.
10	3	8	8	R. c.

Series 2. (*May* 31.)

Trials	L.	R.	1st choices	
1	3	8	8	R. c.
2	8	3	8	L. c.
3	$\frac{1}{2}$	3	$\frac{1}{2}$	L. w.
4	3	8	8	R. c.
5	3	$\frac{1}{2}$	3	L. c.
6	$\frac{1}{2}$	3	3	R. c.
7	8	3	8	L. c.
8	$\frac{1}{2}$	3	3	R. c.
9	3	$\frac{1}{2}$	3	L. c.
10	8	3	8	L. c.

Series 3. (*June* 1.)

Trials	L.	R.	1st choices	
1	8	3	3	R. w.
2	½	3	½	L. w.
3	3	½	3	L. c.
4	8	3	8	L. c.
5	½	3	3	R. c.
6	3	8	8	R. c.
7	8	3	8	L. c.
8	½	3	3	R. c.
9	8	3	8	L. c.
10	½	3	3	R. c.

Summary and notes. Female 1 transferred quite successfully, both food-box doors being unlocked. She chose the 8-inch square each time in the first series of 10 trials of 8 *v.* 3; and in the mixed series—20 trials—made 3 mistakes.

2. Animals trained on 8 *v.* 3 to go to 3, then transferred to 3 *v.* 1.

(*a*) *Male* 3. Required 227 trials before making a sequence of 20 trials all correct on 8 *v.* 3. (April 28–June 3, 1924.)

Transferred to 3 v. 1 (*both food-box doors unlocked*).

Series 1. (*June* 6.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	3	1	3	L. w.
3	1	3	1	L. c.
4	3	1	1	R. c.
5	3	1	1	R. c.
6	1	3	1	L. c.
7	1	3	1	L. c.
8	3	1	1	R. c.
9	1	3	1	L. c.
10	3	1	1	R. c.

Series 2. (*June* 7.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	1	3	1	L. c.
3	3	1	1	R. c.
4	1	3	1	L. c.
5	1	3	1	L. c.
6	3	1	1	R. c.
7	1	3	1	L. c.
8	3	1	3	L. w.
9	3	1	1	R. c.
10	1	3	1	L. c.

Series 3. (*June* 8.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	3	1	1	R. c.
3	3	1	1	R. c.
4	1	3	1	L. c.
5	1	3	1	L. c.
6	3	1	1	R. c.
7	1	3	1	L. c.
8	3	1	1	R. c.
9	3	1	1	R. c.
10	1	3	1	L. c.

Series 4. (*June* 9.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	1	3	1	L. c.
3	8	3	3	R. c.
4	3	1	3	L. w.
5	3	8	3	L. c.
6	8	3	3	R. c.
7	1	3	1	L. c.

Series 5. (*June* 10.)

Trials	L.	R.	1st choices
1	8	3	3 R. c.
2	3	8	3 L. c.
3	3	1	3 L. w.
4	3	1	1 R. c.
5	1	3	1 L c.
6	8	3	3 R. c.
7	3	1	1 R. c.
8	3	8	3 L. c.
9	3	1	1 R. c.
10	8	3	3 R. c.

Summary and notes. Male 3, as did Male 1 and Female 1, transferred quite successfully—both food-box doors unlocked. In 30 trials on 3 *v.* 1, he only made 2 mistakes, that is, he chose the 1-inch square 28 times. In 2 mixed series of 17 trials he made 2 mistakes.

Series 4 was discontinued after the 7th trial on account of the animal getting scared.

(*b*) *Male* 5. Required 150 trials before making a sequence of 20 trials all correct on 8 *v.* 3. (November 9–December 3, 1924.)

Transferred to 3 v. 1 (*door corresponding to* 3-*inch square locked*).

Series 1. (*Dec.* 5.)

Trials	L.	R.	1st choices
1	1	3	1 L. c.
2	3	1	1 R. c.
3	3	1	3 L. w.
4	1	3	1 L. c.
5	3	1	1 R. c.
6	1	3	1 L. c.
7	1	3	1 L. c.
8	3	1	1 R. c.
9	3	1	1 R. c.
10	1	3	1 L c.

Series 2. (*Dec.* 6.)

Trials	L.	R.	1st choices
1	1	3	1 L. c.
2	3	1	1 R. c.
3	1	3	1 L. c.
4	3	1	1 R. c.
5	3	1	1 R. c.
6	1	3	1 L. c.
7	1	3	1 L. c.
8	3	1	1 R. .
9	1	3	1 L. c.
10	3	1	1 R. c.

Series 3. (*Dec.* 7.)

Trials	L.	R.	1st choices
1	3	1	1 R. c.
2	3	8	3 L. c.
3	6	2	2 R. c.
4	3	1	1 R. c.
5	2½	7	2½ L. c.
6	1	4	1 L. c.
7	6	2	2 R. c.
8	2	6	2 L. c.
9	1	4	1 L. c.
10	7	2½	2½ R. c.

Summary and notes. The behaviour of Males 1, 2 and 3 and Female 1 having shown what the animals would do under these conditions of transfer, the wrong door was locked in this and all the remaining transfers, and the animal kept on until it gave a sequence of 20 correct trials or 1 wrong in 20 trials.

Male 5 fulfilled the conditions in the first 20 trials given, making only 1 mistake, that is, he chose the 1-inch square 19 times out of 20 immediately on transfer.

In the mixed series of 10 trials he made no mistakes.

(*c*) *Male* 6. Required 150 trials before making a sequence of 30 correct or 1 wrong in 30 on 8 *v*. 3. (January 16–February 7, 1925.)

Transferred to 3 v. 1 (*door corresponding to* 3-*inch square locked*).

Series 1. (*Feb.* 8.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	1·	3	1	L. c.
3	1	3	1	L. c.
4	3	1	1	R. c.
5	1	3	1	L. c.
6	3	1	1	R. c.
7	1	3	1	L. c.
8	3	1	1	R. c.
9	3	1	1	R. c.
10	1	3	1	L. c.

Series 2. (*Feb.* 9.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	3	1	1	R. c.
3	3	1	1	R. w.
4	1	3	1	L. c.
5	1	3	1	L. c.
6	1	3	1	L. w.
7	3	1	1	R. w.
8	3	1	1	R. w.
9	1	3	1	L. c.
10	3	1	1	R. w.

Series 3. (*Feb.* 10.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	3	1	1	R. c.
3	1	3	1	L. w.
4	3	1	3	L. w.
5	1	3	1	L. w.
6	1	3	3	R. w.
7	3	1	1	R. c.
8	1	3	3	R. w.
9	1	3	3	R. w.
10	3	1	1	R. c.

Series 4. (*Feb.* 12.)

Trials	L.	R.	1st choices	
1	1	3	3	R. w.
2	1	3	1	L. c.
3	3	1	1	R. c.
4	1	3	3	R. w.
5	3	1	1	R. c.
6	1	3	3	R. w.
7	3	1	1	R. w.
8	1	3	3	R. c.
9	3	1	1	R. c.
10	3	1	1	R. c.

Series 5. (*Feb.* 13.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	1	3	1	L. c.
3	3	1	1	R. c.
4	3	1	1	R. c.
5	1	3	3	R. w.
6	1	3	3	R. w.
7	1	3	3	R. w.
8	3	1	1	R. c.
9	1	3	3	R. w.
10	3	1	1	R. c.

Series 6. (*Feb.* 14.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	3	1	1	R. c.
3	3	1	1	R. c.
4	1	3	1	L. c.
5	1	3	3	R. w.
6	1	3	3	R. w
7	3	1	1	R. c.
8	3	1	1	R. c.
9	1	3	1	L. c.
10	1	3	3	R. w.

Series 7. (*Feb.* 15.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	3	1	1	R. c.
3	1	3	1	L. c.
4	3	1	1	R. c.
5	1	3	3	R. w.
6	1	3	3	R. w.
7	3	1	1	R. c.
8	3	1	1	R. c.
9	1	3	1	L. c.
10	1	3	3	R. w.

Series 8. (*Feb.* 16.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	1	3	1	L. c.
3	3	1	1	R. c.
4	3	1	1	R. c.
5	1	3	1	L. c.
6	1	3	1	L. c.
7	1	3	1	L. c.
8	3	1	1	R. c.
9	1	3	1	L. c.
10	3	1	1	R. c.

Series 9. (*Feb.* 17.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	1	3	1	L. c.
3	3	1	1	R. c.
4	1	3	1	L. c.
5	3	1	1	R. c.
6	3	1	1	R. c.
7	1	3	1	L. c.
8	3	1	1	R. c.
9	3	1	1	R. c.
10	1	3	1	L. c.

Series 10. (*Feb.* 18.)

Trials	L.	R.	1st choices	
1	6	2	6	L. w.
2	2	6	2	L. c.
3	8	3	3	R. c.
4	8	3	3	R. c.
5	1	3	1	L. c.
6	6	2	2	R. w.
7	3	8	3	L. c.
8	3	1	1	R. c.
9	2	6	2	L. c.
10	6	2	2	R. c.

Series 11. (*Feb.* 19.)

Trials	L.	R.	1st choices	
1	4	1	1	R. c.
2	2	6	2	L. c.
3	6	2	2	R. c.
4	1	4	1	L. c.
5	2	6	2	L. c.
6	8	3	3	R. c.
7	4	1	1	R. c.
8	3	8	3	L. c.
9	8	3	3	R. c.
10	2	6	2	L. c.

Summary and notes. This animal had had a certain amount of training on the 8 *v.* 3 situation before this series of 150 trials, but on the first occasion he fell ill and training was discontinued.

The animal's behaviour on transfer was somewhat peculiar. The first series he got wholly correct, choosing the 1-inch square each time. Then in Series 2 he began to make mistakes.

In Series 2 all his first choices were correct, that is, he approached and entered the bay containing the 1-inch square each time, but for some reason on 5 occasions he did not enter the food box, but let the door fall, and then went and nosed the other (incorrect door) first. For 5 more series he made mistakes, some of this nature, and some by choosing the

3-inch square first, until finally he gave a sequence of 20 all correct. That is, after his first series which he got wholly correct, he took 60 trials before giving a sequence of 20 all correct. Why this animal after starting so successfully should have developed the peculiar mistake of entering the correct bay, and instead of continuing on into the food box, hesitating a bit, sometimes nosing the door and letting it fall, and then nosing the incorrect door, I am unable to say.

(*d*) *Male* 7. Required 125 trials before making a sequence of 30 trials all correct or 1 wrong in 30 on 8 *v.* 3 (January 16–February 8, 1925).

Transferred to 3 v. 1 (*door corresponding to* 3-*inch square locked*).

Series 1. (*Feb.* 9.)

Trials	L.	R.	1st choices	
1	3	1	3	L. w.
2	1	3	1	L. c.
3	3	1	3	L. w.
4	3	1	1	R. c.
5	1	3	1	L. c.
6	3	1	3	L. w.
7	1	3	1	L. c.
8	1	3	1	L. c.
9	3	1	1	R. c.
10	3	1	1	R. c.

Series 2. (*Feb.* 10.)

Trials	L.	R.	1st choices	
1	1	3	1	L. c.
2	3	1	1	R. c.
3	3	1	1	R. c.
4	1	3	1	L. c.
5	1	3	1	L. c.
6	3	1	3	L. w.
7	1	3	1	L. c.
8	3	1	1	R. c.
9	1	3	1	L. c.
10	3	1	1	R. c.

Series 3. (*Feb.* 12.)

Trials	L.	R.	1st choices	
1	3	1	1	R. c.
2	1	3	1	L. c.
3	3	1	1	R. c.
4	3	1	1	R. c.
5	1	3	1	L. c.
6	1	3	1	L. c.
7	3	1	1	R. c.
8	1	3	1	L. c.
9	3	1	1	R. c.
10	1	3	1	L. c.

Series 4. (*Feb.* 13.)

Trials	L.	R.	1st choices	
1	6	2	1	L. w.
2	3	8	3	L. c.
3	3	1	1	R. c.
4	3	8	3	L. c.
5	6	2	6	L. w.
6	3	1	3	L. w.
7	2	6	2	L. c.
8	2	6	2	L. c.
9	6	2	2	R. c.
10	8	3	8	L. w.

Series 5. (*Feb.* 14.)

Trials	L.	R.	1st choices	
1	8	3	8	L. w.
2	8	3	3	R. c.
3	2	6	2	L. c.
4	6	2	6	L. w.
5	2	6	2	L. c.
6	1	3	1	L. c.
7	8	3	3	R. c.
8	8	3	3	R. c.
9	1	3	1	L. c.
10	6	2	2	R. c.

Series 6. (*Feb.* 15.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	6	2	2	R. c.
3	1	3	1	L. c.
4	3	8	8	R. w.
5	3	1	1	R. c.
6	6	2	2	R. c.
7	2	6	2	L. c.
8	3	8	3	L. c.
9	3	1	1	R. c.
10	2	6	2	L. c.

Series 7. (*Feb.* 16.)

Trials	L.	R.	1st choices	
1	6	2	2	R. c.
2	1	3	1	L. c.
3	8	3	3	R. c.
4	1	3	1	L. c.
5	8	3	3	R. c.
6	6	2	2	R. c.
7	2	6	2	L. c.
8	2	6	2	L. c.
9	3	1	1	R. c.
10	3	8	3	L. c.

Summary and notes. Male 7 required 10 trials before giving a sequence of 20 trials with 1 mistake, that is, in 30 trials he made 4 mistakes, choosing the 1-inch square 26 times in all.

In the mixed series he required 20 trials before making a sequence of 20 trials with 1 mistake. In 40 mixed trials he made 7 mistakes.

3. Animals trained on 3 *v.* 1 to go to 3, then transferred to 8 *v.* 3.

Male 4. Required 206 trials before making a sequence of 30 all correct or 1 wrong in 30 on 3 *v.* 1. (November 9–December 5, 1924.)

Retrained; required 36 trials. (January 16–23, 1925.)

Transferred to 8 v. 3 (*door corresponding to 3-inch square locked*).

Series 1. (*Jan.* 24.)

Trials	L.	R.	1st choices	
1	3	8	8	R. c.
2	8	3	8	L. c.
3	3	8	8	R. c.
4	8	3	8	L. c.
5	3	8	8	R. c.
6	3	8	8	R. c.
7	8	3	8	L. c.
8	3	8	8	R. c.
9	8	3	8	L. c.
10	8	3	8	L. c.

Series 2. (*Jan.* 25.)

Trials	L.	R.	1st choices	
1	8	3	8	L. c.
2	8	3	8	L. c.
3	3	8	8	R. c.
4	3	8	8	R. c.
5	3	8	8	R. c.
6	8	3	8	L. c.
7	3	8	8	R. c.
8	8	3	8	L. c.
9	3	8	8	R. c.
10	8	3	8	L. c.

Series 3. (*Jan.* 26.)

Trials	L.	R.	1st choices	
1	3	8	8	R. c.
2	3	1	3	L. c.
3	2	6	6	R. c.
4	3	8	8	R. c.
5	6	2	6	L. c.
6	3	1	3	L. c.
7	6	2	2	R. w.
8	1	3	3	R. c.
9	8	3	8	L. c.
10	2	6	6	R. c.

Series 4. (*Jan.* 27.)

Trials	L.	R.	1st choices	
1	4	1	4	L. c.
2	6	2	6	L. c.
3	3	8	8	R. c.
4	8	3	8	L. c.
5	1	4	4	R. c.
6	1	3	3	R. c.
7	2	6	6	R. c.
8	3	1	3	L. c.
9	4	1	4	L. c.
10	3	8	8	R. c.

Summary and notes. Male 4 was not transferred on December 6 owing to lack of time, so he was retrained later—January 16–23.

Male 4 fulfilled the conditions of transfer in the minimum number of trials. In the first 20 trials on 8 *v.* 3, he chose the 8-inch square correctly 20 times, and in the mixed series of 20 trials he made one mistake.

4. Animals trained on 4 *v.* 1, to go to 4, then transferred to 6 *v.* 2.

Female 3. Required 186 trials before making a sequence of 30 trials all correct, or 1 wrong in 30 on 4 *v.* 1. (January 16–March 1, 1925.)

Transferred to 6 v. 2 (*door corresponding to 2-inch square locked*).

Series 1. (*March* 2.)

Trials	L.	R.	1st choices	
1	2	6	6	R. c.
2	6	2	6	L. c.
3	6	2	6	L. c.
4	2	6	6	R. c.
5	6	2	6	L. w.

Series 2. (*March* 3.)

Trials	L.	R.	1st choices	
1	6	2	2	R. w.
2	2	6	6	R. c.
3	6	2	6	L. c.
4	6	2	6	R. c.
5	2	6	6	R. c.
6	2	6	6	R. c.
7	6	2	6	L. c.
8	6	2	6	L. c.
9	2	6	6	R. c.
10	2	6	6	R. c.

Series 3. (*March* 4.)

Trials	L.	R.	1st choices	
1	2	6	6	R. c.
2	6	2	6	L. c.
3	2	6	6	R. c.
4	6	2	6	L. c.
5	2	6	6	R. c.
6	2	6	6	R. c.
7	6	2	6	L. c.
8	2	6	6	R. c.
9	6	2	6	L. c.
10	6	2	6	L. c.

Summary and notes. Female 3 trained on 4 *v.* 1, was transferred to a 6 *v.* 2 situation. In 25 trials she chose the 6-inch square 23 times.

The first series was discontinued after trial 5 on account of the animal being frightened.

Summary of the Results of the First Transfer.

Animal	Trained on	To go to	No. of trials required	Transferred to	Trials	1st choices	'Wrong' food-box door
1. Male 1	3 *v.* ½	3	127	8 *v.* 3	10	8-in. 9 times	Unlocked
„ 1	3 *v.* ½	3	127	Mixed 8 *v.* 3, 3 *v.* ½	10	1 mistake	„
„ 2	3 *v.* ½	3	305	8 *v.* 3	16	R. position habit	„
„ 2	3 *v.* ½	3	305	8 *v.* 3	24	8-in. 21 times	Locked
„ 2	3 *v.* ½	3	305	Mixed 8 *v.* 3, 3 *v.* ½	10	1 mistake	„

Animal	Trained on	To go to	No. of trials required	Transferred to	Trials	1st choices	'Wrong' food-box door
Female 1	3 *v*. ½	3	280	8 *v*. 3	10	8-in. 10 times	Unlocked
,, 1	3 *v*. ½	3	280	Mixed 8 *v*. 3, 3 *v*. ½	20	3 mistakes	,,
2. Male 3	8 *v*. 3	3	227	3 *v*. 1	30	1-in. 28 times	,,
,, 3	8 *v*. 3	3	227	Mixed 3 *v*. 1, 8 *v*. 3	17	2 mistakes	,,
,, 5	8 *v*. 3	3	150	3 *v*. 1	20	1-in. 19 times	Locked
,, 5	8 *v*. 3	3	150	Mixed 3 *v*. 1, 8 *v*. 3, 6 *v*. 2, 4 *v*. 1, 7 *v*. 2½	10	No mistakes	,,
,, 6	8 *v*. 3	3	150	3 *v*. 1	90	Required 70 trials before choosing 1-in. 20 times	,,
,, 6	8 *v*. 3	3	150	Mixed 1 *v*. 3, 8 *v*. 3, 6 *v*. 2, 4 *v*. 1	20	2 mistakes	,,
,, 7	8 *v*. 3	3	125	3 *v*. 1	30	1-in. 26 times	,,
,, 7	8 *v*. 3	3	125	Mixed 3 *v*. 1, 3 *v*. 8, 6 *v*. 2	40	7 mistakes	,,
Male 4	3 *v*. 1	3	206	8 *v*. 3	20	8-in. 20 times	,,
,, 4	3 *v*. 1	3	206	Mixed 8 *v*. 3, 3 *v*. 1, 6 *v*. 2, 4 *v*. 1	20	1 mistake	,,
4. Female 3	4 *v*. 1	4	186	6 *v*. 2	25	6-in. 23 times	,,

(*b*) *Second Transfer*.

This consisted in transferring all the animals trained on the 8 *v*. 3 situation to an 8 *v*. 3 situation in which the illumination in both bays was equalized. This was done by cutting down by means of a resistance the lamp which illuminated the 8-inch square until the illumination in the bay lighted by the 8-inch square was equal to that in the bay lighted by the 3-inch square, as measured by several readings with a luxometer.

It is to be noted that this was not the only change produced in the situation. The actual brightness of the 8-inch square was changed. The 8-inch square was now relatively less bright than the 3-inch square, although the bays which they lighted were now of equal brightness. This would naturally follow from cutting down the lamp which illuminated the 8-inch square. By this means a situation was produced on which one—difference in illumination—of the possible means of discrimination was eliminated, and the other—difference in size of the squares—kept.

This experiment took place after the first transfer experiment. The animals were retrained, or at least re-acquainted with the 8 *v*. 3 situation for a short time, and then transferred to the 8 *v*. 3 situation size alone.

Male 3. Retrained on 8 *v.* 3, taking 90 trials. (October 13–24, 1924.)
8 v. 3 *size only* (*wrong door locked*).

Series 1. (*Oct.* 24.)

Trials	L.	R.	1st choices	
1	3	8	8	R. w.
2	8	3	3	R. c.
3	3	8	8	R. w.
4	3	8	8	R. w.
5	8	3	3	R. c.
6	3	8	8	R. w.

Series 2. (*Oct.* 25.)

Trials	L.	R.	1st choices	
1	3	8	3	L. c.
2	8	3	3	R. c.
3	3	8	8	R. w.
4	3	8	3	L. c.
5	8	3	3	R. c.
6	3	8	3	L. c.
7	8	3	3	R. c.
8	8	3	3	R. c.
9	3	8	3	L. c.
10	3	8	8	R. w.

Series 3. (*Oct.* 26.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	3	L. c.
3	8	3	3	R. c.
4	8	3	3	R. c.
5	3	8	3	L. c.
6	3	8	3	L. c.
7	8	3	3	R. c.
8	3	8	3	L. c.
9	3	8	8	R. w.
10	8	3	3	R. c.

Series 4. (*Oct.* 27.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	3	L. c.
3	3	8	3	L. c.
4	8	3	3	R. c.
5	3	8	3	L. c.
6	8	3	3	R. c.
7	8	3	3	R. c.
8	3	8	3	L. c.
9	8	3	3	R. c.
10	3	8	3	L. c.

Summary and notes. The first 4 trials of Series 1 were ordinary 8 *v.* 3 trials (he did them correctly) the 8-inch square being cut down for the first time in trial 5—Series 1 above. The animal was somewhat perturbed and fell into a right position habit for the rest of the trials. In the next series he recovered and only required 10 trials—Series 2—before making a sequence of 20 trials with 1 mistake.

Male 5. 8 v. 3 *size only* (*wrong door locked*).

Series 1. (*Dec.* 7, 1924.)

Trials	L.	R.	1st choices	
1	3	8	8	R. w.
2	3	8	3	L. c.
3	8	3	3	R. c.
4	8	3	3	R. c.
5	3	8	3	L. c.
6	8	3	3	R. c.
7	3	8	3	L. c.
8	8	3	3	R. c.
9	8	3	3	R. c.
10	3	8	8	R. w.

Series 2. (*Dec.* 8.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	3	R. c.
3	8	3	3	R. c.
4	3	8	3	L. c.
5	3	8	3	L. c.
6	8	3	3	R. c.
7	3	8	3	L. c.
8	8	3	8	L. w.
9	3	8	3	L. c.
10	8	3	3	R. c.

Summary and notes. Male 5 was only given 20 trials on the 8 *v.* 3 situation size alone. He made 3 mistakes, choosing the 3-inch square correctly 17 times.

Male 6. 8 v. 3 *size only* (*wrong door locked*).

Series 1. (*Feb.* 27, 1925.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	8	R. w.
3	8	3	3	R. c.
4	3	8	8	R. w.
5	3	8	8	R. w.
6	8	3	3	R. c.
7	3	8	8	R. w.
8	8	3	3	R. c.
9	3	8	8	R. w.
10	3	8	3	L. c.

Series 2. (*Feb.* 28.)

Trials	L.	R.	1st choices	
1	3	8	8	R. w.
2	3	8	3	L. c.
3	8	3	3	R. c.
4	3	8	3	L. c.
5	8	3	3	R. c.
6	3	8	8	R. w.
7	8	3	3	R. c.
8	8	3	3	R. c.
9	3	8	3	L. c.
10	3	8	8	R. w.

Series 3. (*March* 1.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	8	R. w.
3	3	8	8	R. w.
4	8	3	3	R. c.
5	8	3	3	R. c.
6	3	8	8	R. w.
7	3	8	3	L. c.
8	8	3	3	R. c.
9	3	8	8	R. w.
10	3	8	3	L. c.

Series 4. (*March* 2.)

Trials	L.	R.	1st choices	
1	3	8	3	L. c.
2	8	3	3	R. c.
3	3	8	3	L. c.
4	3	8	8	R. w.
5	8	3	3	R. c.
6	8	3	3	R. c.
7	3	8	3	L. c.
8	3	8	8	R. w.
9	8	3	3	R. c.
10	8	3	3	R. c.

Series 5. (*March* 3.)

Trials	L.	R.	1st choices	
1	3	8	3	L. c.
2	8	3	3	R. c.
3	8	3	3	R. c.
4	3	8	3	L. c.
5	3	8	3	L. c.
6	3	8	3	L. c.
7	8	3	3	R. c.
8	8	3	3	R. c.
9	3	8	3	L. c.
10	8	3	3	R. c.

Series 6. (*March* 4.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	3	L. c.
3	3	8	3	L. c.
4	8	3	3	R. c.
5	8	3	3	R. c.
6	3	8	3	L. c.
7	3	8	3	L. c.
8	8	3	3	R. c.
9	3	8	3	L. c.
10	8	3	3	R. c.

Summary and notes. Male 6 required 40 trials before giving a sequence of 20 trials all correct on the 8 *v.* 3 size alone situation; as may be seen from Series 1 he developed a right-hand position habit.

Male 7. 8 *v.* 3 *size only* (*wrong door locked*).

Series 1. (*Feb.* 27, 1925.)

Trials	L.	R.	1st choices	
1	8	3	8	L. w.
2	3	8	3	L. c.
3	8	3	3	R. c.
4	3	8	3	L. c.
5	8	3	3	R. c.
6	8	3	3	R. c.
7	3	8	3	L. c.
8	3	8	3	L. c.
9	8	3	3	R. c.
10	3	8	3	L. c.

Series 2. (*Feb.* 28.)

Trials	L.	R.	1st choices	
1	3	8	3	L. c.
2	8	3	3	R. c.
3	3	8	3	L. c.
4	8	3	3	R. c.
5	8	3	3	R. c.
6	8	3	8	L. w.
7	3	8	8	R. w.
8	3	8	3	L. c.
9	8	3	3	R. c.
10	8	3	8	L. w.

Series 3. (*March* 1.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	8	3	3	R. c.
3	3	8	3	L. c.
4	3	8	3	L. c.
5	8	3	3	R. c.
6	3	8	3	L. c.
7	8	3	3	R. c.
8	8	3	8	L. w.
9	3	8	3	L. c.
10	3	8	3	L. c.

Series 4. (*March* 2.)

Trials	L.	R.	1st choices	
1	8	3	3	R. c.
2	3	8	3	L. c.
3	8	3	3	R. c.
4	8	3	3	R. c.
5	3	8	3	L. c.
6	3	8	3	L. c.
7	8	3	3	R. c.
8	3	8	3	L. c.
9	8	3	3	R. c.
10	3	8	3	L. c.

Summary and notes. Male 7 required 20 trials before making a sequence of 20 trials with only 1 mistake.

Summary of Results of the Second Transfer.

Animal	Trained on	Transferred to	No. of trials given	Mistakes
Male 3	8 *v.* 3	8 *v.* 3 size only	36	1st 16, 6 wrong; last 20, 1 wrong
„ 5	8 *v.* 3	8 *v.* 3 „	20	3 wrong
„ 6	8 *v.* 3	8 *v.* 3 „	60	1st 40, 14 wrong; last 20, 0 wrong
„ 7	8 *v.* 3	8 *v.* 3 „	40	1st 20, 4 wrong; last 20, 1 wrong

(10) THEORETICAL CONSIDERATION OF THE RESULTS OF THE TRANSFER EXPERIMENTS.

The experiments which have been described above follow closely on the well-known experiments of Prof. W. Kohler[1], and in general their results agree with his.

Before going on to the theoretical discussion of the experiments, it is necessary to define the sense in which certain terms will be employed.

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[1] See *e.g.* Koffka, *Psych. Bull.* 1922, pp. 542–3.

(a) *The Relational Significance of a Stimulus.*

Any two 'stimuli,' *A* and *B*, presented together will of course possess certain relations one to the other. Among these may be spatial relations —*A* to the left of *B*; *B* to the right of *A*, etc. But the two stimuli will also possess other relations, those which exist between the qualities which all physical objects possess, *e.g.* *A* is brighter than *B*, *B* is larger than *A*, *A* is longer than *B*, etc. These relations are objective, that is, they are given in perception. I perceive that *B* is larger than *A*, or that *A* is brighter than *B* as directly as I perceive that *B* is green, *e.g.*

When, therefore, the term 'relational significance' is used with reference to a stimulus, what is implied is that it is possible to say of it one at least of the phrases quoted above; that, perceptually, (for us) it is brighter than, greater than, etc., the other stimulus. Whether a stimulus which has a certain relational significance for us has the same relational significance for an animal, must, of course, be determined experimentally if it can be determined at all.

(b) *The Functional Significance of a Stimulus.*

By functional significance is meant that it is possible to describe an animal's behaviour with reference to a stimulus by saying that "this stimulus means, go to this"—positive functional significance; or, "this stimulus means, avoid this"—negative functional significance. Whether a stimulus will come to possess positive or negative functional significance in an experiment depends on the method of training adopted by the experimenter.

(c) *The First Transfer Experiment.*

With these two definitions in mind let us consider the situations which were presented to the animals. In each situation there were two components or stimuli—two bays differently illuminated and containing two illuminated square areas differing in size, the larger area being in the more brightly illuminated bay. Each bay with its square may be termed a stimulus. Some of the animals were trained to go to the brighter bay, some to the darker bay.

Now, applying our terms to a concrete situation, we can make the description of that situation more definite as follows. Take, for example, the 3 *v.* ½ situation. We can now say of this—stimulus 3 (bay + square) possessed relational significance *B* (brighter than) with reference to stimulus ½, while stimulus ½ possessed relational significance *D* (darker than) with reference to 3.

Again, it can be said that stimulus 3 (square alone) possessed relational significance *G* (greater than), while stimulus ½ possessed relational significance *S* (smaller than)[1].

Now consider the transfer situation, which was, in this particular case, the 8 *v.* 3 situation. In this it is obvious that stimulus 3 now possesses relational significance *D* and *S*, and no longer possesses relational significance *B* and *G*, which are now possessed by stimulus 8.

In training on the 3 *v.* ½ situation, stimulus 3 was made to acquire positive functional significance, and stimulus ½ negative functional significance.

On transfer it was discovered that stimulus 3 no longer possessed its original positive functional significance, but possessed negative functional significance, while stimulus 8 possessed positive functional significance. These are the facts which require explanation. The close connection between relational and functional significance will be observed at once, and it is in the nature of this connection that the explanation lies.

Still sticking to a concrete case we can explain the connection as follows. In the 3 *v.* ½ situation the animals were compelled to go to stimulus 3. [There are rather fundamental points involved in this statement. Of course, in one sense, the animals were not compelled to go to either stimulus. They were perfectly 'free' to nose the door and remain in the bay containing the ½-inch square all night if they so chose. But from the point of view adopted in the above discussion of the learning behaviour, the animals were compelled to go to the 3-inch square to satisfy their motivating tendency. This, it seems, is the only sense in which the word compel can be used in this connection, physical compulsion being absent. In the previous discussion the assumption was made that out of several conflicting tendencies to behaviour present in the Emotional Phase a selection was effected—owing to the nature of the conditions of experimentation, nature of the tendencies involved, etc.—in the Phase of Directed Effort of a single dominant tendency. It was assumed that this tendency was the food-seeking tendency. So that when I say that the animals were compelled to go to stimulus 3, I mean that, assuming the food-seeking tendency to be operative, this tendency could acquire satisfaction only by the animals going to stimulus 3.]

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[1] Only for us, experiment showed that *in this particular situation* no stimulus possessed relational significance of *size* for the animals.

The animals in the 3 *v.* ½ situation were compelled to go to stimulus 3. Stimulus 3, therefore, came to acquire positive functional significance for the animals, came to mean "go to this." But we have seen that stimulus 3 possessed relational significance *B*—*i.e.* "this stimulus which is brighter than that stimulus." The assumption seems thoroughly justified that the functional significance of stimulus 3 comes to be not merely "go to this," but "go to this which is brighter than that."

On this assumption the behaviour of the animals in the transfer experiments is easily explicable. The animals will inevitably go to that stimulus in the transfer situation which possesses the same relational significance as the stimulus to which they had been accustomed to go in the old situation, and they will do this because of the intimate connection which has resulted between the functional significance of the stimulus and its relational significance, a connection which might be described by saying that the functional significance of the stimulus inheres in or is carried by its relational significance in the situation. The functional significance is no longer merely "go to this," but "go to this which possesses relational significance *B*." This intimate connection is of course established during training on the 3 *v.* ½ situation. Learning might, indeed, be said to consist in the establishment of this connection. When, therefore, an animal is transferred, the stimulus in the new situation which possesses the same relational significance as the old positive stimulus, will also possess the same functional significance for the animal.

Put in a general form these conclusions may be summarized as follows. In a situation in which stimulus *A* possesses relational significance *R*, and stimulus *B* relational significance *r*, and in which by training stimulus *A* comes to possess functional significance *F*, this functional significance *F* is bound up with, carried by or inheres in the relational significance *R* of stimulus *A*.

And in the event of any change in such a situation so that stimulus *A* no longer possesses relational significance *R* but *r*, the animal will go to that stimulus which now possesses relational significance *R*.

The gradual apprehension by the animal that "go to this" is really "go to this which possesses relational significance *R*" is the function of a capacity which, in *Gestalt* terminology, is probably meant by the term 'insight.'

More generally, again, the conclusion may be drawn that in many cases of animal learning it is impossible to account adequately for the animal's behaviour in terms of reaction to isolable elements or sensation units considered in abstraction from each other. The conclusion seems justified that, in many cases at least, the determining conditions of animal behaviour are to be found in what has been called the relational significance of stimuli. The assumption is made that the relations involved are perceptual data. That the fact expressed by saying, "*A* is brighter than *B*," "*A* is smaller than *B*" is given in perception just as the fact expressed by saying "*A* is green," "*B* is square" is so given.

(*d*) *The Second Transfer Experiment.*

The second transfer experiment also becomes easily explicable on the above assumptions. In this the animals were transferred from the normal 8 *v.* 3 situation to an 8 *v.* 3 situation in which the intensity of light in the two bays was made equal, leaving only the difference in size of the two squares. The result was that the animals were able to transfer to this new situation. In the normal 8 *v.* 3 situation the '3 stimulus' possessed positive functional significance. It also possessed relational significance *D* (darker than) and *S* (smaller than).

The results would seem to indicate that during training on the normal 8 *v.* 3 situation, the important factor was the brightness difference, while the transfer experiment would indicate that even during the behaviour to the normal situation the *S* factor also had some effect on the animal. The positive functional significance of the '3 stimulus' was carried then by both relational factors *D* and *S*.

We must assume that in behaviour to the normal 8 *v.* 3 situation the *D* factor was the most important, was dominant, while the *S* factor played a less decisive rôle. But when the *D* factor was eliminated the animals were not helpless, as the relational *S* factor of 'stimulus 3' also carried in some measure its positive functional significance. The result was, therefore, that the animals quickly adjusted themselves to the changed situation.

These facts would seem to indicate that in a complex situation in which there is more than one discriminable relational factor, *i.e.* one which may carry functional significance, a certain selection does take place—of course not a conscious selection.

That is, it is possible to say that a certain relational factor is of more importance than another, though, if certain conditions demand it, each may be used on the exclusion of the other.

(11) SUGGESTIONS FOR A FURTHER EXPERIMENT.

An attempt to investigate further the relative importance of the various relational factors in a complex situation might prove interesting. Such an investigation might be conducted along the following lines.

A situation must be arranged in which the two 'stimuli' differ in respect of several relational factors, for example, brightness, size, pattern. Animals must be used which have previously been shown to be capable of discriminating by all three factors—brightness difference, size difference, pattern difference. These conditions being obtained, the investigation might proceed as follows:

Group A of the animals trained on the complex situation—the two 'stimuli' differing in respect to all three factors—could then be transferred to a situation in which two factors were eliminated and one remained, *e.g.* pattern difference.

Group B, similarly trained, could be transferred to a situation possessing only brightness difference.

Group C, similarly trained, could be transferred to a situation possessing only size difference.

The readiness and ease of transfer would then be an indication of the relative importance of each of these three factors in the learning of the complex situation.

If, for example, the animals transferred easily to the situation possessing only brightness difference, transferred less quickly to the situation possessing only size difference, and with difficulty or not at all to the situation possessing only pattern difference, then it could be said that in the learning of the complex situation the brightness factor was of more importance than the size factor, and both of them of more importance than the pattern factor.

(*Manuscript received* 20 *November*, 1926.)

CRITICAL NOTICE

BY H. R. DE SILVA.

An Introduction to the Theory of Perception. By Sir J. H. PARSONS. Pp. 254. Cambridge, The University Press. 1927. Price 18*s.*

Psychology would be fortunate indeed if other medical specialists as outstanding in their own fields as Sir J. H. Parsons is in that of vision, would contribute to psychological knowledge books with as complete and scholarly a treatment of experimental literature as his *Introduction to Colour Vision* and the volume cited above. Although the work under review is more general in subject-matter than the earlier one, it is written in the same careful manner and is made invaluable to the student and prospective research worker by the inclusion of numerous carefully chosen and reliable footnote references.

The title is somewhat misleading, because the book is fundamentally concerned with a summary and discussion of evidence in favour of the theory of functional levels in the nervous system. Three levels are distinguished—protopathic, epicritic, and syncritic. The lowest, protopathic, or *dyscritic* (as Parsons prefers to call it), comprises a primitive undifferentiated mechanism consisting of *perceptions brutes* or "blooming buzzing confusions." These lower neural centres are controlled by an epicritic mechanism which is much more discriminative. At a still higher level a syncritic mechanism is hypothecated which has the function of integrating epicritic phenomena.

There is nothing original in the theory of levels which is hardly more than a modified presentation of Hughlings Jackson's three levels as enunciated in 1889. The value of the book, however, lies in the fact that it brings together systematically under one cover a large mass of important but scattered results from anatomy, physiology, and psychology. The authors most frequently cited are Sherrington, Head, Rivers, Holmes, Ariens Kappers, Frank Allen, and McDougall.

Although the author recognizes three levels in the nervous system, he devotes most of his space to illustrating the universality of a dual epicritic and *dyscritic* (protopathic) mechanism in exteroceptive sensations, principally vision. Proper treatment of the syncritic mechanism "must be left to the future." While it is quite obvious that studies made upon the highest or syncritic mechanism of the human organism, are not so readily correlated with nerve processes, nevertheless an author who is "convinced that psychology as a science can only be developed upon a sound biological basis" should mention that, in addition to *Gestalt* psychology, traditional experimental psychology has attempted to work out some of the laws governing the functioning of the syncritic mechanism by numerous experiments upon imaging, attention, reaction time, and higher thought processes in general. Psychology may be dependent upon progress in biology, especially as concerns the dyscritic mechanism, and to a less extent the epicritic mechanism, but as regards the highest or syncritic mechanism, biology has been of little service in the past and seems likely to play an equally secondary rôle in the near future.

The first four chapters, which are of great interest to the psychologist, are unfortunately disposed of very briefly. Chap. I, on "The Genesis of Perception," and Chap. II, on "Receptors," give the reader an introduction to the theory of levels from an evolutionary and developmental standpoint. We are told that "The dawning consciousness is probably a sentiency with greater affective than cognitive tone" and therefore "complete knowledge of an event seems to be attainable only by a backstroke from a higher level" to a lower affective level. Consciously this backstroke process seems to partake of the nature of primitive knowing. Again, "The fundamentally prepotent receptors of the living organism were interoceptors to which the exteroceptors became more and more accessory as they underwent higher differentiations, differentiations which were largely in the interest of protection."

In Chap. III, "Instinct," the author points out that the consciousness accompanying the second and subsequent performances of instinctive activities, is due to experience from the first and subsequent activities, and is caused by backstroke from the cortex. Thus intelligence is based on the data of instinct and in turn reacts upon instinct by a backstroke process. The original emotional states are feeble things compared with full-grown emotions on subsequent occasions. In these the content of the affective state has been elaborated and enriched by backstroke and by associated memory.

Chap. IV, on "Perpetual Patterns," discusses the work of Sherrington, Kohler, Wertheimer and G. E. Müller. The bipartite division of protopathic and epicritic is given more significance in reference to psychological patterns. Protopathic patterns are those of vital import; epicritic patterns have cognitive import. The protopathic or crude undifferentiated mass perception occurred earlier in the evolutionary history of the organism than did epicritic or discriminative perception. At still higher levels within the epicritic stage there is a growth of the forward-looking tendency germinal in protopathic awareness, and a retention of the protopathic character of pleasant or unpleasant, which become elaborated into an emotional state. In experiences giving rise to typical instinctive responses the affective tone looms large in consciousness, with relative inhibition or suppression of cognition. In higher syncritic stages, interest is essentially cognitive and affective tone is relatively inhibited or suppressed. Consciousness emerged in the animal species "for the purpose of synthesizing and integrating the apparently incongruous elements of which consciousness is made up, extero- and interoceptive impulses and so on." There are "hierarchies of integrations, and when a lower level integration has proved sufficiently serviceable to any given species it becomes part and parcel of the make up of the individuals of the species" or "in common terms is relegated to the unconscious." Parsons accepts the view that in the organic world many phenomena are present which cannot be explained as a mere algebraic summation of constituent parts. Various psychological concepts are discussed, such as *ergänzende Gestaltproduktion*, or arousing of meaning from incomplete data; *Umformung* or rectifying capacity; pregnancy of pattern, or tendency of presentations to arouse good patterns, *i.e.* relatively simple and familiar patterns.

We are given the facts and theory concerning cutaneous sensibility in Chap. v. The author attempts to show that although the distinction of protopathic (*dyscritic*) and epicritic is due to Head and Rivers, the distinction rests upon surer ground than they put forward.

In Chaps. vi and vii the evidence from comparative anatomy in favour of dyscritic mechanism is reviewed at length. We are told that protopathic and epicritic impulses are conducted by different fibres in the peripheral nerves. Sherrington's two principles of plurireceptive summation and interference are invoked to explain the integration of impulses from different modalities. These chapters are in marked contrast to the rest of the book in that they present material which has only distant bearing on the problem of perception. Although the author seems to address himself to psychologists elsewhere in the book, in these sections he overwhelms the non-medical reader with difficult diagrams and unnecessary details from neurology and anatomy, mainly from Kappers.

Chap. viii takes up "Perception of Space," both among the lower animals and among men. It is pointed out that there is a correspondence between the macula of the retina and the tip of the finger in that they both function in fine discrimination. In the orthodox fashion, space perception is shown to depend upon numerous cues, none of which, however, enter consciousness.

In Chap. IX is summarized the most important experimental work on the perception of movement. Evidence is cited to show that the perception of movement is an example of *erganzende Gestaltproduktion.* The more meanings that can be read into a perceptual pattern, the more illusions of movement are likely to be experienced. The author's point of view upon the problem may best be illustrated by the following passage: "We shall find it more fruitful to confine ourselves to eliciting the laws which govern the perception of movement than to attempt to discover the cause."

The distinction between *dyscritic* (protopathic) and epicritic vision is dealt with in detail in Chaps. X, XI, XII, and XIII. Unfortunately, only about ten pages are devoted to auditory, tactile, gustatory, and olfactory sensations.

Outstanding characteristics of the book are the author's spirit of fairness and the way in which he backs up his argument with pertinent facts. Nevertheless, *Introduction to the Theory of Perception* impresses the reviewer as being much too ambitious a title for a book whose central idea is a defence of the validity of a dual system of epicritic and protopathic mechanisms. Many psychologists would be quite willing to welcome any physiological explanations which could be consolidated with attested psychological facts to promote a systematic interpretation of the human mind, but the concepts mentioned by Parsons—as for example, pluri-receptive summation, interference, *ergänzende Gestaltproduktion,* and backstroke process—in their present form do little to clarify the mystery of perception, and give at best an extremely patchy "biological basis for a science of psychology." As an effort to interpret perception physiologically the book is unconvincing; as an exposition of the experimental data bearing on the dichotomy of epicritic and protopathic sensibilities, it forms a valuable contribution to physiological psychology.

PUBLICATIONS RECENTLY RECEIVED

Educational Psychology. Its Problems and Methods. By CHARLES FOX, M.A. London, Kegan Paul. Pp. xiii + 380.

Books on educational psychology have usually been very disappointing. The tendency has been to try to write a simplified psychology for the teacher with all the psychology left out. Hence a serious book on educational psychology, based on the results of recent experimental work, can hardly fail to be of value. One is consequently predisposed to think highly of Fox's book. To a considerable extent the predisposition is confirmed by reading the book. One is, however, occasionally jarred into a more critical attitude by coming across sentences here and there which are rhetorical rather than scientific, and which have obviously behind them a somewhat emotional reaction on the part of the author towards the opinions expressed or the views criticized. This is notably the case in the chapters on "Mental Imagery," "Habit Formation," "Psycho-Analysis," and "Mental Tests."

In the first of these chapters the author dismisses contemptuously the "psychological abortion of the 'eidetic image,'" and exhibits a dogmatism which detracts to some extent from the value of a very interesting chapter. In the chapter on "Psycho-Analysis," and particularly in the discussion of the "Sexual Theory," he shows an emotional bias which is alien to the spirit of scientific enquiry. Here and there in the other two chapters the same kind of thing occurs, and one cannot help, as we have said, being jarred into criticism where one would fain read with enjoyment and commendation.

This occasional lack of balanced and discriminating judgment is also found elsewhere in the book, as soon as one becomes sufficiently awake to the necessity of a critical examination of the author's statements before accepting them. Thus in the first chapter, though he does not definitely commit himself to the theory of the inheritance of acquired characteristics, his inclinations are clearly in that direction, and he seems prepared to accept the current reports of Pavlov's experiment with white mice at their face value. Again, in discussing the question whether impulses can be acquired, and whether a habit is a driving force, he appears to be satisfied that he has adduced evidence that disposes of McDougall's view, when he has merely cited facts that neither McDougall nor anyone else would or could either overlook or deny. The discussion of the theory of formal discipline and the criticism of mental testing are sometimes marred in the same way.

In spite of these blemishes, however, the book can be warmly recommended, as not only useful, but stimulating to an unusual degree. The chapters on "Habit Formation," "Memory" and "Mental Discipline" are probably the most important chapters from the educational point of view, and they are in the main excellent. All show the influence of the *Gestalt* psychology, but there are many original observations by the author, which, involving as they frequently do, somewhat controversial points, are so much the more interesting and provocative of thought. These lend a piquancy to the discussion, which one rarely meets, or expects, in a scientific treatise.

For the most part, the author's style is lucid, and the argument straightforward.

Occasionally, however, the precise point and force of a sentence is very difficult to determine. For example, on p. 82, the author says: "What makes one hesitate more than ever in accepting such statements is the repeated assertion of persons who claim to have very clear and vivid visual images that they could draw from them if only they knew how to draw; whereas artists who *can* draw assert that their 'imagery is so clear that if they had been unable to draw they would have unhesitatingly said they could draw from it.'" Doubtless the author had a reason for, and a meaning in, this sentence. The writer, however, has tried it every way and fails to find either the reason or the meaning. It must be granted that this kind of thing does not happen very often in the book, but it does now and again. Nevertheless, the book as a whole is a valuable one, and cannot fail to be of great service to the educationist and teacher at the present time. J.D.

Humanizing Education. By SAMUEL D. SCHMALHAUSEN. New York: The New Education Publishing Co. 1926. Pp. 343.

The author regards this book as a plea for "critical mindedness" against credulity. It appears that to be critical minded is to be an opponent of all established institutions and customs, and to be credulous is to admit virtue in things as they are. In a long series of short essays Mr Schmalhausen demonstrates that there is much to be said for this point of view. He is perhaps less critical in some directions than in others, but he is at any rate very fully convinced that American educational methods are bad. He writes exuberantly, and shoots in so many directions that he is fairly certain to have made a number of hits. He seems to enjoy a good whole-hearted slaughter, but it is hard to imagine anything very definite being built up out of his frame of mind. The whole book wants well cooling down, and then, perhaps, it might do a certain amount of good.

Experimental Psychology. By MARY COLLINS and JAMES DREVER. London: Methuen & Co. 1926. Pp. viii + 315. 8*s.* 6*d.* net.

This book is intended to deal with "the ground that a student might reasonably be expected to cover in a first year's university course in Experimental Psychology." Certainly a student who diligently covered the whole course in a year would have to be fairly busy and alert. If anything, it is a little overloaded, but that is a good fault for it can be amended by any teacher to suit his own purposes. The treatment throughout is clear, and if it is not exciting that is perhaps a merit in a somewhat 'heady' subject. There is no great novelty of arrangement. The psycho-physical methods are described early, as they ought to be, and then, as is still the general custom, come chapters on the special senses, on perception, and so, through suggestion and feeling to imagery and the higher thought processes. Probably a better arrangement is to put the detailed special sense work much later and to begin with topics in which less detailed physiological knowledge is available or required. It seems doubtful whether at present studies in the "psycho-galvanic reflex" ought to be introduced into a single year's work. Appendices deal with "The Psycho-organic system," "Short methods of calculating correlation," and "The effects of drugs." Further references are given in a selected bibliography. The book may fairly be said to be a success. It does not claim or achieve any marked originality; but it does what it sets out to do in a very workmanlike way. It is less attractively produced than could be desired.

A First Laboratory Guide in Psychology. By Mary Collins and James Drever. London: Methuen & Co. 1926. Pp. viii + 108. 5*s.* net.

Probably every teacher who has charge of students in an experimental psychological laboratory prefers to arrange their courses himself to suit the resources of his laboratory and the size of his classes. But here at any rate is a sensible and sufficiently comprehensive first year's laboratory work. It is suited to the needs of large classes, since it does not demand the use of much apparatus and many of its experiments are of the 'group' variety. The experiments proposed are throughout clearly and adequately described. The book ought to be very useful also to the solitary student who has to make his own laboratory.

The Effects of Music. Edited by Max Schoen. London: The International Library of Psychology, Philosophy, and Scientific Method; Kegan Paul, Trench, Trubner and Co., Ltd. 1927. Pp. ix + 275. 15*s.* net.

This book contains several papers that were submitted for a competition conducted by the American Psychological Association in 1921 for research on the effects of music. Undoubtedly the best is that of Myers (published in this *Journal*, Vol. xiii), although it is hardly more than a beginning in a rather neglected subject. Ortman's essays on different types of musical appeal are also of value, though unfortunately based on the assumption that, genetically, sensation precedes perception, and perception precedes imagination.

As regards the rest, it is more than remarkable that the musical appreciation of all women students at American colleges is strikingly uniform, that the uniformity is more marked for subjects tested by one investigator than for those tested by different investigators, and that their mode of appreciation does not bear any resemblance to that of an ordinary educated musical English listener. The researches consist almost wholly of unjustifiable statistical treatment of marks given by the subjects to different gramophone records for pleasure and emotions, the emotions being provided by the experimenter, not by the subject. There is practically no mention of the value or goodness that determines the aesthetic appreciation of most English listeners; (one's heart goes out to a single exception, a music teacher who would give inconsistent results because, quite naturally, she "disliked this type of music"). Further, all the investigations except those of Ortman are vitiated by the use of the gramophone, whose reproductions are bad now but were very much worse six years ago. The only experimental conclusion of any value arrived at is that popular music tends to pall more rapidly than classical.

Those who are interested in the subject might do well to read the book if only to see the extraordinary way in which American students (or shall we say American psychologists) appreciate music. Incidentally, it is also interesting to learn that the "Nutcracker Suite" was written by Grieg.

The Psychology of Murder: A study in criminal psychology. By Andreas Bjerre. Translated from the Swedish by E. Classen. London: Longmans Green & Co., Ltd. 1927. Pp. xii + 164. 9*s.* net.

The writer of this fascinating book was professor of Legal Philosophy at the University of Dorpat during the four years preceding his death in 1925. For many years previously he had made a close study of criminals in the Central Prison at Långholmen, Stockholm, endeavouring, with keen psychological insight, to understand and to analyse their inner life.

As a result of this study Prof. Bjerre suggests a tentative classification of murderers into three groups according to the *weakness* which is apparent in their psychic life. He gives full sketches of the lives of three typical murderers.

He is convinced that fear of punishment does not act as a deterrent, and that sex is *never* of more than secondary importance in explaining the behaviour of criminals.

This volume, written by one who was not primarily a psychologist, is a distinct contribution to the psychology of crime.

The Art of Thought. By Graham Wallas. London: Jonathan Cape. 1926. Pp. 320.

Into this recent book Mr Graham Wallas has packed the secrets of a life of thought which has been more than ordinarily creative. The simple, yet adequate discussion of the process of creative thought—a most difficult subject—is a real help to anyone desiring to rise from an apprentice thinker into a competent craftsman.

Man, it seems, is increasing his control over nature and his own destinies, but is not increasing his power of thought. Want of intellectual control exists in politics, philosophy and art, particularly. Man requires, therefore, both more effective thinking on particular problems, and an improved art of thought. But from psychology, whence one would naturally expect help on such a problem, comes little aid because of the mechanistic conception of instinct as "a power," driving reason as a "machine." Mr Wallas would substitute for this the "hormist" conception of the human organism as an imperfectly integrated combination of elements, each retaining initiative of its own, while co-operating with the rest.

Further, it is necessary to realize that consciousness varies from full consciousness to unconsciousness. The thinker must train himself to observe his less conscious psychological experiences, for it is on this 'fringe' that creative ideas are likely to appear.

Creative effort may be divided into four stages: Preparation, Incubation, Illumination and Verification. "At the Preparation stage we can consciously accumulate knowledge, divide up by logical rules the field of inquiry, and adopt a definite "problem attitude." In Verification we can consciously follow out rules like those in preparation. At the incubation stage we can consciously arrange, either to think on other subjects, or to rest. If we are consciously to control the Illumination stage we must observe the "fringe of consciousness" psychological events which precede and accompany the flash of Illumination.

The book then discusses the relation of "Thought and Emotion," "Thought and Habit," etc. Mr Wallas urges the value of a time stimulus, as producing the habit of warming up, which may be combined with a place stimulus to the same effect. But creative work should not become the slave of habit. A break now and again furnishes a fresh intellectual departure.

In the last three chapters, and the best is "The Thinker at School," Mr Wallas offers a criticism of the present day educational methods both in America and England, and enquires what would happen to help or hinder the education of a Plato, or Milton, or Darwin, if such a person were now born into our society.

The book is a curious and delightful mixture of analysis and opinion. Mr Wallas draws much valuable material from his experience both in America and England. Moreover, he is, like every great teacher, inspiring. The book has an emphasis much needed in present day education, an emphasis on the individual and the development of his creative powers, rather than on the average. E. D. H.

Art and Instinct. By Prof. S. ALEXANDER. The Herbert Spencer Lecture at Oxford, 23 May, 1927. At the Clarendon Press. 1927. Pp. 23.

Prof. Alexander claims the existence of a specific aesthetic emotion having its origin in the instinct of construction. He appeals for precedent in assigning the artistic impulse to a specific instinct to Spencer, who derived art from the play impulse, and to Burke, who traced it to the instincts of self-preservation and sex. "Between animal constructiveness and artistic production there is an intermediate stage of handicraft." Constructiveness becomes art when, ceasing to be practical, it becomes contemplative; "when it is pursued for its own sake, and the constructed object...is used as the satisfaction of the constructive passion itself."

One criticism may be phrased in the form of a question. Is not art described more explicitly in terms of the subconscious impulses and desires which are discoverable by the technique of present day psychology? Prof. Alexander is probably quite right: Dante's *Divine Comedy* may be the outcome of the instinct of constructiveness. It is perhaps more explicitly and immediately understood as the expression of the unfulfilled desires of an exiled poet and of his baffled love for Beatrice Portinari.

E. D. H.

Psychopathology. By BERNARD HART. Cambridge: University Press. 1927. Pp. vii + 156. 7*s.* 6*d.* net.

This volume contains (*a*) the three Goulstonian lectures on "The development of psychopathology and its place in medicine," which Dr Hart delivered before the Royal College of Physicians in 1926; (*b*) an appendix to Lecture II entitled "The conceptions of the subconscious, coconscious, and unconscious"; and (*c*) two papers, the one on "The psychology of rumour" and the other on "The methods of psychotherapy," which were published in 1916 and 1918 respectively.

The result is a rather disjointed presentation of the subject of psychotherapy with the repetitions inevitable in any collection of papers which have been prepared with different specific aims. The Goulstonian lectures give a lucid and interesting account of the development of psychopathology, in which the limitations of the earlier methods are pointed out and those of the School of Freud are carefully criticized. For the general reader, and for the would-be student of psychotherapy, far too much attention is given to the estimation of the place to be assigned to psychopathology in medicine. The paper on Rumour is of interest, but, as it stands, it does not justify the space it occupies in a volume entitled *Psychopathology*. In the last paper—which contains much that has already been set forth in the lectures—the methods of "affective therapeutics" (suggestion), persuasion, and analysis are compared, and the conclusion is reached that practical psychotherapy involves an admixture of these principles.

When I took up this book I hoped that at last we had a small book which would give a clear, well-balanced, unbiassed and yet critical account of the schools of psychopathology, a book for which there is a demand and which Dr Hart is so well qualified to write. This collection of lectures and papers, excellent though they be, does not satisfy the need.

A Textbook of Psychiatry for students and practitioners. By D. K. HENDERSON and R. D. GILLESPIE. Humphrey Milford: Oxford University Press. 1927. Pp. x + 520. 18*s*. net.

The authors realise that the time has come when a knowledge of mental diseases should form a part of the equipment of the already heavily laden general practitioner, and have succeeded in their attempt to supply the necessary information in a readable form. Those interested will find in this book accounts of the aetiology and symptoms of mental illness, and directions as to the examination and treatment of patients which will be of great utility.

Physique and Character: An Investigation of the Nature of Constitution and of the Theory of Temperament. By E. KRETSCHMER. Translated by W. J. H. Sprott. London: International Library of Psychology, etc.; Kegan Paul, Trench, Trubner & Co., Ltd. 1925. Pp. xiv + 266. 15*s*. net.

The book is divided into two parts, the first dealing with Physique, the second with the Temperaments. After a description of his method and some review of existing classifications, Kretschmer finds that three types of physique are important. These he calls 'asthenic'—exhibiting in general a "deficiency in thickness combined with an average unlessened length"—the 'athletic,' and the 'pyknic'—displaying in general a rounded figure, with a tendency to fat and little muscle or bone relief. These types are to be studied in their relation to psychiatry and Kretschmer starts from the general broad classification by Kraeplin into 'circular insanity' and schizophrenia. Broadly speaking—general statistics are presented—the asthenic and athletic types gravitate towards dementia praecox, the pyknic types towards manic depressive (or 'circular') insanity.

In the second part Kretschmer designates as cycloid and schizoid those temperaments which alternate between sickness and health showing certain fundamental dementia praecox symptoms and light 'circular' psychoses. The cycloid type is "sleek and uncomplicated," the schizoid has "surface and a depth." Both are described in great detail and with many illustrations. Then comes a chapter on "the average man," amusing to read but much less good than many an old book of 'characters.' A readable chapter on geniuses leads to a final brief "theory of temperaments." Kretschmer here leads up to a biological view but actually adds little to his general descriptive matter which forms the bulk of the book.

The book is beyond doubt in many ways a striking one. It is very uneven, a great many of the hints, suggestions and generalizations being bright and stimulating; but not over-impressive on the statistical side. The author is very fond of broad schemes and classes and one gets the impression that an observer who had not invented the classes might find it somewhat difficult to put people into them. Still when all is said, as a method of approach to very complicated phenomena, the schemes outlined have great interest. And at the present stage of knowledge it may be that over-simplification is more of a virtue than a vice.

The Measurement of Emotional Reactions. By DAVID WECHSLER. Archives of Psychology. Ed. R. S. WOODWORTH. No. 76. New York. 1925.

This monograph gives an account of researches on the psychogalvanic reflex conducted by the author and others. It discusses, though not as fully as is desirable, the physical and physiological sides of the phenomena, as well as their psychological interpretations. Any student who wishes to get a general knowledge of work done on the psychogalvanic reflex up to about the end of 1924 should certainly consult this monograph with its excellent bibliography.

ABC of Jung's Psychology. By JOAN CORRIE. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1927. Pp. x + 85. 3*s.* 6*d.* net.

The outlines of Jung's psychology are sketched in this small book in four short chapters. Jung's conceptions of the structure of the mind and of its functioning is outlined, with the help of four diagrams, in the first two chapters; the last two chapters deal with disturbances of the mind and the significance of dreams. The manuscript was read by Dr Jung himself prior to publication.

The subject-matter is necessarily very condensed: slightly fuller treatment, especially of the chapter dealing with the disturbances of the mind, would have increased the value of the book considerably. It is, however, recommended as an introduction to Jung's psychology.

The Animal Mind. By MARGARET FLOY WASHBURN. New York: The Macmillan Co. 1926. Pp. xiii + 431. 12*s.* 6*d.* net.

It is encouraging that a third edition of this admirable and well-known textbook should be called for. Dr Washburn has added some account of recent German work on sensory discrimination, some description of the important work from the physiological department at Amsterdam, considerable changes in the sections on perceptual reactions with a recognition of the influence of the '*Gestalt*' school, and a new final chapter attempting a brief summary view of the essential characteristics of animal mind at different stages of organization. The bibliography has been extended. For its size the book is a remarkable storehouse of facts, and these have generally been well selected and are clearly described. Some further extension of the final chapter, or perhaps the introduction of similar chapters at the end of every section, would make it easier for a student to see some sort of order in the mass of details presented.

The Interpreter Geddes, the Man and his Gospel. By AMELIA DEFRIES. London: Routledge. 1927. Pp. xiii + 334. 10*s.* 6*d.* net.

This is a most enthusiastic piece of hero-worship. From the psychological point of view it is exceedingly interesting, not only for the vigour, intensity and strain of the writing; for the comments upon every possible movement of men that has taken place and does, or possibly may take place; for the idealism and vivid belief in the value of life; but equally for the multitudes of diagrams of a visual order which are supposed to clarify ideas about all manner of subjects. The book is rather over-long, but it contains much good material for reflection. There is a brief foreword by Rabindranath Tagore, a preface by Mr Zangwill, appreciation by numerous writers in the course of the book, and in conclusion appreciation by five well-known friends. There emerges from it all an effective and somewhat volcanic personality to whom the author submits with a splendid, and perhaps somewhat embarrassing, devotion.

The Psychology of the Methodist Revival. By SYDNEY G. DIMOND. Published by Humphrey Milford at the Oxford University Press. Pp. xiv + 296. 10*s.* 6*d.* net.

A Psychological review of this book could never degenerate into a scathing commentary, for, although it is quite unoriginal to the psychologist, it does not claim to be more than an attempt to find application for modern psychological theories in a special context—that of the Methodist Revival. And this it does admirably. However, the sort of 'Behaviourism' in which Mr Dimond indulges in Chap. III is, if anything, worse than that of Prof. Watson. If we are going to be 'Behaviourists' let us be *Behaviourists.* One sentence from the "Behaviourist study of the Mind of John Wesley" is that he "*never lost the consciousness that he was the child of destiny*" (p. 53).

As a historical study the book is a contribution of some value to the literature of the subject. The chapters containing analysis of the historical contexts of the Methodist Revival are very interesting and ably written. But, on the whole, like so many other psychological studies of religious questions, it boils down to an emulsion of psychology and religion.

Magnetism and Magic. By BARON DU POTET DE SENNEVOY. Translated and edited by A. H. E. LEE. London: George Allen and Unwin, Ltd. Pp. 157. 6*s.* net.

"This book is an abbreviated translation of *La Morgue Dévoilée*...Paris...1852." It is a book that no trained thinker would take very seriously, except in rather uncritical moments. But all the same it is the sort of book that could be prescribed for the reading of materialistic-minded scientists with some advantage, as an antidote to the view that science is able to 'explain away' various mysteries.

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MENTAL UNITY AND MENTAL DISSOCIATION[1]

BY WILLIAM BROWN, Oxford and London.

IT is important to realize that the problem of mental unity and dissociation is in direct relationship to the problem of unity and dissociation in the physical and physiological spheres. The point of view from which we should first approach it is that of biology. The general tendency throughout evolution seems to be towards the building up of totalities or wholes, in which each separate activity takes place in relation to the whole. The general conception is not that of a purely mechanistic scheme in which we begin with particles of matter and consider how they interact with one another to produce a more and more complex system, but one in which there is a guiding unity from the beginning. We may indeed, as metaphysicians, assume that there is a guiding unity of the entire universe. But short of such an ultimate generalization we find that observation itself reveals this tendency towards a progressive development and multiplication of unities, in relation to which individual activities occur.

In one sense, biology may be regarded as the most fundamental of the sciences, and even the simpler physical and chemical activities occur as parts of systems, which may be likened to organisms in having an environment to which their reactions are adjusted. This biological point of view needs for its completion the psychological point of view, which is not something distinct from the biological, but a continuation of it. Psychology is a completion of biology as a science, and gives further meaning to it. The unity of the organism becomes more intelligible when we think of it as a mental, and in part a conscious, unity. It is a unity in plurality. Physically, the organism is a unity of parts in spatial relation to one another; psychologically, it is a system of mental tendencies in relation to one another. What we find physically is a reaction to an environment in the form of reflex action, simple or conditioned. Psychologically, response to external stimulation is the satisfaction of conation, and, at higher levels, satisfaction of desire, etc. Biologically, it is the struggle for existence, with all that this involves. Psychologically, it is a conscious striving first of all for something that the individual does not know—towards a goal which he gradually realizes, gradually learns to understand as he achieves it or fails to achieve it. However helpful the biological concepts of tropisms and conditioned reflexes may be as explanatory factors, the psychological point of view throws further light upon the situation. The ultimate factor that has to be considered is something that is *purposive*—a general striving—which factor has to be assumed in order that the theory of conditioned reflexes will work.

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[1] The presidential address to Section J (Psychology) of the British Association for the Advancement of Science, Leeds, 1927.

Unity of mind, then, is something which develops. In its general form it is there from the beginning, but it only exists in relation to a multiplicity. There is a one-and-many relationship from the beginning. Moreover, the individual himself is, to some extent, an abstraction. He belongs to a species. He has a history, and his history is, in part, the history of the entire species. The history of the species is part of the history of organic evolution, and the history of organic evolution is part of the history of the Universe—if, indeed, we may think of the Universe itself as having a history. But within the individual experience there are partial activities that may struggle with one another just as individual members of a species may compete with one another, and in this nisus or striving towards more complete unity and greater complexity and more adequate adaptation to environment, the process of dissociation shows itself as essential and normal. Right from the beginning we must realize that dissociation is just as normal and necessary as association. The mind as it grows must be able to reject, and also must be able to segregate one activity from another. Different activities must be insulated from one another to a great extent within the organism, just as in an electrical machine there must be insulation of the wires. This insulation is what is meant by normal dissociation or disjunction. And from the pathological point of view there can be disturbance in both respects—disturbance of association and disturbance of dissociation. Experiences may become associated in such a way as to blur the clearness of vision of the individual as regards appearances and values, just as, more obviously, dissociation may go beyond its proper function and tend to destroy the unity or totality towards which normal striving is directed.

The earlier associationist doctrine of psychology was unsatisfactory because, among other things, it failed to distinguish between the process of experiencing or the act of experiencing, and the content or object of experience; and its aim seems to have been to describe the mind as a sort of mosaic of contents of experience joined up to one another according to the laws of association by contiguity and similarity. But association is primarily between the acts of experience. These acts of experience are differentiations of the fundamental striving—that which Spinoza called the *conatus in suo esse perseverare*, "the striving to persist in one's own being." This striving has an object. There is always an object—the environmental changes which the individual has to face—and corresponding with the complexity of the environment there is a complexity developing within this general striving, forming a complex system of conations. This conation involves the two other well-known aspects of consciousness, viz. cognition or awareness, and feeling-tone. Associationist psychology erred in failing to allow due weight to the conative side, and in attempting to range the different kinds of feeling on a level with the different kinds of objective experience. But even according to the associationist scheme, dissociation or disjunction was necessary for a complete explanation of normal mental activity. Side by side with the principles of association by contiguity and similarity, there was the principle of dissociation by varying concomitance. Only by such a principle could the important processes of discrimination, comparison and abstraction be brought within the circle of associationist doctrine. The general scheme of explanation was wrong, in that it dealt with contents of experience when it should have been dealing with acts of experience; and the general scheme of mental activity which we put in the place of this associationist scheme is that of systems of mental tendencies included within, and in subordination to, a wider system—tendencies towards knowledge and action, and involving feeling. One such scheme of explanation is that in terms of instinctive-emotional dispositions organized within sentiments, with the sentiments in their turn subordinated to one all-inclusive sentiment or master-interest. A sentiment is an organization of emotional dispositions centred about the idea of some object. What are organized are not the presentations or representations, not the contents of experience primarily, but the processes or acts of experience. The tendencies of experience, and the activity and organization of these tendencies, bring with them an organization of the objects; and so our memories, which are retained and which are used for the retention of past experiences, fall into systems because the acts of experience corresponding to those memories fall into systems. And from that point of view we see that the dissociations of memory—the gaps in the memory continuum, known as amnesias—find their true significance in the segregation of corresponding acts of experience.

The question then arises, if we reject the associationist scheme according to which the mind is built up of presentations and representations cohering together in systems, are we to regard the unity of the mind as an aggregation of mental activities and tendencies? Are we to put in place of our mosaic of mental contents a collection or a colony of mental tendencies? The reply is 'No,' because we do not have these mental tendencies separate from one another—just coming together and cohering, just as we do not have the mental contents coming together and cohering. Both subjectively and objectively, we must assume at the beginning a generalized striving and mental tendency, with a generalized objective—*e.g.* 'the great big buzzing confusion' of which William James speaks as the world of the new-born babe—a continuum of sensation and movement on one side, and a continuum of mental striving on the other. The general mental striving which we assume at the beginning becomes gradually differentiated in relation to the needs of the organism, and the demands made upon the organism by its environment—a differentiation superimposed upon the differentiations accumulating in the history of the race, and handed on from generation to generation in the form of instinctive endowment and inherited aptitudes. The individual inherits not only separate instincts, but also the tendency towards an organization of those instincts. He inherits the beginnings of sentiments, as well as instincts. He is already a one-and-many unity, with his mind a plurality of part-tendencies and processes, and his task in life is to carry that organization to a higher stage. The demands made upon him by his environment bring with them movements in two directions—in the direction of greater unity and greater organization, and also of greater complexity—a greater degree of differentiation and discrimination. Discrimination is necessary because he has not only to accept, but also to neglect or reject, and this neglecting or rejecting involves dissociation, just as acceptance involves association.

As regards dissociation in pathological cases, the writers of the last generation thought of this in terms of associationist psychology, and in the doctrine of Prof. Pierre Janet one finds that standpoint still apparent. In his description of cases of hysteria and multiple personality, he implies a general background of explanation, according to which personality may be regarded as a synthesis of mental presentations some of which can be split off from the main mass. This view is similar to the colonial view of personality which we find in the writings of Ribot. But if we remind ourselves of the fact that experience involves an act of experiencing, we see that the situation is rather different. The power of recall is an essential aspect of conscious memory. On the other hand, unconscious memories are unconscious or latent mental activities directed towards past events. They are not passive, but involve a certain amount of mental energy. And so we pass from Pierre Janet's theory to the theory of Prof. Sigmund Freud, and we find that the dissociation which is taken as a fact in Janet's theory is explained in Freud's theory in terms of mental conflict and repression. These memories become inaccessible to the individual because the mental tendencies corresponding to them are in conflict with other tendencies of the individual and incompatible with his more fundamental interests. So they are extruded by an active process of repression—they are barred from consciousness.

That process of repression and extrusion, though pathological in these cases, need not be necessarily so. We must not look upon extrusion itself as essentially pathological. It is because we have restricted the word dissociation so much to the pathological side that we find it so incomprehensible to us. It is because we have thought too much in terms of mental unity that cases of multiple personality seem to be inexplicable. Actually the most normal mind is a multiplicity. We are all many selves. We have to face the world from many different angles. We have many different interests. Interests in the most normal mind may conflict and be incompatible with one another. And it is a condition of mental health that such conflict can be resolved by elimination or by a higher synthesis. What makes the dissociation of multiple personality pathological, is that the elimination is not complete—that dissociation in normal mental activity is a successful rejection, and that dissociation in a pathological case is unsuccessful—is an incomplete and therefore an unsuccessful rejection. A tendency that is pathologically repressed is, as it were, rejected and accepted at the same time—rejected by clear consciousness, but still clung to by the mind.

It is misleading to look upon the problem of mental dissociation and multiple personality as something standing by itself, as if we understood mental unity and were perplexed by the appearance of multiplicity. Multiplicity is an aspect of the normal mind, just as much as unity is, and unity needs explaining just as much as multiplicity does. Those two problems must be solved together, and kept in relation to one another all through.

Many of the classical cases of multiple personality are fully explained along these lines. They are cases of alternating personality with reciprocal amnesia, as it is called, in which each personality is unable to recall the experiences of the other. The two individuals *A* and *B* alternate with one another. *A* has his own system of memories and, when he disappears and makes way for *B*, *B* has his system of memories distinct from the memories of *A*. We may explain this in terms of two general systems of interests which are mutually incompatible and in conflict with one another. As a rule, one part of the personality is more fundamental, *i.e.* more stable, than the other. But difficulty arises in cases where one personality is shut up within its own memories and experiences, while the other personality has access to those memories as well as to its own. *A* may have his system of memories but be quite ignorant of *B*, except from indirect evidence, whereas *B* not only has a special set of memories, but also has direct knowledge of *A*'s memories, thoughts and feelings. This is a difficult problem which needs to be faced. We find an analogous, though not identical, situation in most cases of deep hypnosis. When a patient passes into the hypnotic state, he may remain fully aware of his waking consciousness, and may have free access to the memories of his waking self. But on awaking he does not, as a rule, remember his hypnotic experiences. The relation is one-sided. The hypnotic personality is acquainted with the waking personality, and all its memories, but the waking personality is not acquainted with the hypnotic personality. The range of the hypnotic personality is wider than the range of the waking personality. This similarity between one-sided multiple personality and the hypnotic personality is significant, when we remember that such cases have been investigated by the hypnotic method. Pierre Janet, Morton Prince and others used the hypnotic method in studying cases of multiple personality, and the criticism has been made against them that in doing so they were manufacturing personalities—that the personalities were artefacts produced by the method. Everyone recognizes that these investigators are psychologists of exceptional ability and circumspection and honesty of purpose, thoroughly trained and alive to the possibilities and difficulties of their method. We cannot dismiss their observations as false observations, or as misunderstandings on their part. But we must nevertheless allow for the influence of the process of hypnosis in the result, and as contrasted with that earlier period of investigation—the hypnotic period in psychopathology—we find that, now that hypnosis is seldom used and has been replaced by deep analysis, cases of multiple personality are not on record. The psycho-analysts to-day seem to have no such cases to report. Moreover, if we contrast the very large number of cases of severe nervous disturbance caused by the late war with the absence of cases of multiple personality there, we may feel still more impressed by the argument that it was the persistent use of the hypnotic method that was mainly responsible for the complexity of most of the earlier cases reported[1].

The movement of thought is always towards system and unity. Thought abhors arbitrary distinctions. Thought is baffled by cases of multiple personality because they are so different from the ordinary cases of everyday life. If we can build a bridge between one group of cases and the other, then we may feel that we are likely to have not only a more satisfying but a true explanation of the situation.

We must therefore approach the question of dissociation from the normal side—as manifested in a relatively normal mind. No mind is completely normal, since no mind completely solves its problems from day to day, and it is the failure to solve mental problems which is one of the general causes of the symptoms of psycho-neurosis and mental disease. Dissociation and multiple personality are not to be contrasted with association and mental unity. Pathological dissociation should be contrasted with the dissociationist processes of the normal mind. It should be regarded as a failure of the normal process of dissociation.

The unity of the normal mind, although it is there from the beginning, is a striving towards a more and more complete association; it constitutes an urge to a greater and greater degree of completeness of systematization and inclusiveness, but it is never really complete. In the most normal mind there is a falling away from complete unity. There is in the activity of this unitary mind not only a normal process of disjunction or dissociation, but also a certain degree of abnormal dissociation. In cases of multiple personality this abnormal dissociation has become so pronounced as to be apparent to the whole world. The process of deep analysis or psycho-analysis fails to reveal cases of thorough-going multiplicity, and the reason is that the process itself is a process of unification. As the individual is being analysed, the failures of adaptation in his past life are cleared up, so that his mind is enabled to work more and more normally. Analysis is not a good term for this process. It is more than analysis, it is a process of self-revelation or *autognosis*. The individual learns to know himself better, and in the process of analysis there is actual development of the mind going on. There is a development in the direction of the normal and the unitary. Any dissociation that is encouraged by the method is a normal dissociation, not an abnormal dissociation. It is only another expression of the same truth when we say that repressions are overcome in the process of analysis, because repressions are pathological dissociations—dissociations that are not complete and not thorough-going.

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[1] Cases of extensive amnesia, fugues, etc., were numerous during the war; but the first aim of the army doctors in the battle areas was to remove these amnesias and re-associate the patients as quickly as possible, so that the latter might be either returned to the line or sent down to the base with the minimum of delay.

In contrast with this process of autognosis, the process of hypnotic investigation carries with it a tendency to abnormal dissociation. A person who is easily hypnotized is a person who is already, to some extent, dissociated; in hypnotizing him we take a wedge, as it were, and drive it into his mind and split him up still more. No wonder the results give us an appearance of dissociation; but it would be very dangerous for us to take these results at their face value and draw inferences from them as to the structure of the normal mind, or even of the mind of the person we have been experimenting with. This general line of criticism seems valid as against such a theory as that of Prof. W. McDougall in the last chapter of his *Outline of Abnormal Psychology*, in which he works out a theory of the Self as a system of monads which form a hierarchy, in which there is one dominant monad, the conscious self, and a whole number of subsidiary monads that are, in a normal mind, adequately subordinated to the chief monad and are in relation to the chief monad through telepathy[1]; but in a case of multiple personality one of these subsidiary monads may break loose and become insubordinate. This is an ingenious theory, and it may be true, but in the present state of our knowledge it would seem to be a case of explaining *obscurum per obscurius*. Telepathy may be a fact, but it is something about whose *conditions* we know next to nothing, and therefore not very suitable to take as a fundamental factor in an explanation of the working of the mind.

The mind can act at different levels on different occasions and under different circumstances. In many of the classical cases of multiple personality, the subsidiary personalities represent a regression to more juvenile forms of behaviour and of ethical valuation. This is clearly apparent in 'Sally Beauchamp' of the Miss Beauchamp case, and in the 'B' personality of the 'B.C.A.' case (Morton Prince). Such manifestations are not accurately described as 'split-off' personalities. Indeed, any spatial metaphor is inappropriate. In other cases the tendency to dramatization, natural to the human mind, may play an important part. Mutually incompatible ideals of character may be simultaneously or alternately aimed at, and identifications in early life, based on love and admiration for relatives, etc., may introduce incompatibilities which reveal themselves under circumstances of stress in later years as the grounds of pathological dissociation.

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[1] Unlike the monads of Leibniz, which 'have no windows,' and are in a relation of pre-established harmony.

As regards the problem of the *removal* of pathological mental dissociation in hysterical patients, much was learnt from the wide range of cases dealt with during the war. While treating shell-shock cases in the field in France, I found that a large proportion of the cases showed a more or less extensive amnesia for events that had occurred immediately after the shell explosion. These patients were easily hypnotized, and under light hypnosis the lost memories could be immediately restored. But I soon discovered that if I recalled at the same time the terrifying emotion that had originally belonged to these experiences, there was a tendency for the accompanying hysterical symptoms—deafness, mutism, tremors, paralysis, contractures, etc.—to disappear spontaneously, without the necessity of giving explicit suggestions to this end. The more complete I made the working-off of the emotion, or the 'ab-reaction,' to use Breuer's original term, the more complete was the recovery. In cases seen by me previously in England, I had also restored lost memories by light hypnosis, but had not produced intense emotional revival and had not seen collateral symptoms disappear. But again, towards the end of the war, I was seeing more chronic cases in Scotland, and then found that amnesias of longer standing could be cleared up with accompanying ab-reaction of the emotion of fear; and that with the ab-reaction there was observable that tendency for the collateral symptoms to disappear at the same time, such as I had observed so frequently in France.

These are observed facts, and I endeavoured to show in a paper in the *British Medical Journal* some years ago[1] that they could be explained in terms of a theory of re-association. The amnesic patients fresh from the trenches showed a two-fold dissociation, namely (1) a dissociation of the memory of events immediately following upon the shell explosion from memories of earlier and later parts of the patient's life; and (2) a dissociation of these memories, as mere intellectual awareness, from the accompanying emotional reaction of fear—tremors, sweating, mutism, paralysis, etc.—which are of a physiological nature. The physical reaction of fear, thus dissociated from its psychical counterpart, had become relatively permanent instead of being transitory. The patient no longer felt the emotion of fear—at least, of just that fear which the shell explosion had aroused—but did show its physical manifestations in the form of hysterical symptoms. By re-arousing the whole of the lost experience in all its emotional vividness I overcame *both* dissociations. The physical manifestations became linked up with their psychical counterpart, and this in its turn was linked up with earlier and later memories of the patient's life. In this way the mind was completely resynthesized, and the physical symptoms came once more under the sway of the entire mind (the complete personality) and could disappear.

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[1] "Hypnotism, Suggestion, and Dissociation," *British Medical Journal*, June 14, 1919. My theory and practice of ab-reaction was developed in relation to *hysterical patients of a special type*; it is only as applied to such cases that I advocate the method, although my observations (on many hundreds of patients of this type) have also a wider bearing on general psychological theory. The matter is fully discussed in my *Psychology and Psychotherapy* (London: Edward Arnold & Co., 1922), especially in Chapters I, VI and VII.

In addition to ab-reaction, I advocate the thorough thinking out of the whole psychological situation by the patient, so that he may be brought eventually to understand himself adequately. This is the process of autognosis, or self-knowledge. The patient is encouraged to obtain as objective a view of his entire mental condition as is possible.

C. G. Jung[1] has explained the beneficial effects of ab-reaction in terms of 'transference.' By transference is meant the emotional rapport (conscious and unconscious) which springs up between patient and physician, and which enables the patient to live again, in the course of an analysis, through earlier experiences of his life in relation to the physician, and thus become freed from their harmful effects. The following case illustrates how ab-reaction can bring benefit without the factor of transference coming in. It is a case of a man of considerable education, who had for some years suffered from obsessive fear, the origin of which he could not fathom. He would wake up in the morning with this fear weighing upon his mind. After reading about the method of ab-reaction, as used in treating shell-shock patients, he thought that he would try to cure himself by a similar method. He endeavoured to recall earlier and earlier memories of his past life, using the method of concentration—to all intents and purposes producing a light degree of self-hypnosis. At length he seemed to get this memory: it was half a memory, half a waking vision. He seemed to be in a sort of native compound in India. He experienced intense heat, such heat as he never remembered experiencing in his life before, and seemed to see a black kid lying on the ground with its throat cut and blood pouring out of the wound. He felt intense terror as he went through this experience. This terror grew and grew 'like a bubble.' It got bigger and bigger and at last burst, and all at once the fear began to subside again and eventually disappeared, and he remained free of it afterwards. As far as one could make out—he came and told me of it afterwards; I had not treated him at the time—he had cured himself of the fear by bringing up this memory. He could not be certain that the memory was a real memory, but thought that it probably was, because he had lived in India up to the age of two, when he left for England and had not returned since. It was thus probable that it was a real experience; if not so in all its details, the central kernel of the experience was probably real, and its recall was effective in curing him. It will be noticed that he did not ab-react this experience in relation to another person. He was not in a doctor's consulting-room, telling the doctor what he could remember. He was alone. He had not even gone to a doctor beforehand, so that it could not be described as a transference towards the doctor in the latter's absence. He had not applied to any doctor for treatment at that time. He came on to me afterwards, simply to talk the matter out still further, and to learn whether he had been working on the right lines, and how he should proceed in order to ensure that the fearsome experience should not return. An example like this is a refutation of the view that the only beneficial effect of ab-reaction is the transference. Transference is indeed often the chief factor of cure, and in many ab-reaction cases transference is an additional factor. But an example like this shows that ab-reaction by itself has therapeutic value, in contradiction to Jung's view[1].

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[1] He writes: "It must, above all, be emphasized that it is not merely the rehearsal of experience that possesses an unconditional curative effect, but the rehearsal of experience *in the presence of the physician.*" *Brit. Journ. Med. Psych.* II, 1921.

Ab-reaction of repressed emotion sweeps away the repression, and so frees energy which had been previously needed to hold the repressed memories apart from the rest of the mind and away from clear consciousness. This freed energy is thus put once more at the general disposal of the personality. The previous 'fixation' of this repressing energy and its deviation from the common fund of energy of the personality probably explains, to some extent, the feeling of fatigue that generally accompanies a psycho-neurosis.

The unitary personality, as an organization of mental activities and mental powers, is not static but dynamic, and is in process of development throughout life. Although it carries with it, as a physical correlate, a unitary working of the brain and of other parts of the body, this does not necessarily involve complete dependence upon the latter for its continued existence. The question of personal survival of bodily death is one which can be intelligibly and scientifically put, and which is in theory answerable along the lines of scientific observation and inference. The investigations carried out by the Society for Psychical Research during the past fifty years are of this nature, and the Society's results and provisional hypotheses can rightly claim a place in modern psychological science. Nevertheless, if due allowance is made for the possible working of such factors as conscious or sub-conscious fraud, telepathy between the living, and chance coincidence, the scientific evidence for personal survival of bodily death is not very strong.

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[1] I record this case in *Talks on Psychotherapy*, University of London Press, Ltd., 1923, pp. 39–41.

For more convincing reasons (apart from the pronouncements of revealed religion) in support of this belief we still have to turn to philosophy, and in modern philosophical theories of *value* we find arguments that are far from negligible[1].

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[1] I have set out arguments from theory of value in my *Mind and Personality*, University of London Press, Ltd., 1926, pp. 309–318.

THE SUB-STRUCTURE OF THE MIND

By C. SPEARMAN.

I. *Conformation by Simple 'Chance' or 'Sampling'* (pp. 249–251).
II. *The Case of Extremely Numerous Elements* (pp. 251–252).
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I. CONFORMATION BY SIMPLE 'CHANCE' OR 'SAMPLING.'

As is well known, there have been very extensive efforts to discover the structure underlying mental abilities by means of the correlations observed between them. And such a means of investigation has now become doubly important, because it is being applied more and more to the other great side of human nature also, that of character and temperament. The following paper describes an attempt to carry this method a little further in respect of abilities, with a hope that much of the results obtained may be found serviceable for the study of character and temperament likewise.

Now, the correlations between any abilities not intrinsically very similar nor incidentally connected has been observed to present the striking feature of 'hierarchy.' The strict criterion of this is that all the 'tetrad differences' should (save for errors of sampling) equal zero*. Another criterion—which possesses at any rate approximate validity—is that all the 'inter-columnar correlations' (again, after eliminating the influence of sampling errors) should tend to perfect unity†.

Among the endeavours to explain this hierarchy, one view which has attracted much notice consists in attributing it to what has been designated as 'chance' or—less clearly, I think—'sampling.' Hitherto, unfortunately, the investigation of this theory has been much hampered by the notion that it is opposed to and a rival of the theory called that of 'two factors' or '*g*' and '*s*.' In truth, however, there is nothing to prevent both theories being true at the same time. Each has to stand or fall on its own merits*.

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* That is to say, we should get $r_{ap}.r_{bq} - r_{aq}.r_{bp} = 0$ whichever of the variables a, b, p, or q may be made to stand for. See *The Abilities of Man*, by the present author, 1927, App. p. iii.

† The 'inter-columnar correlation' is the correlation between any two columns in the ordinary square table of correlations. See *ibid.* p. viii.

As regards the theory of 'chance,' all really turns on how this term is conceived. Let us then begin by banishing from our minds the too-prevalent tendency to regard it as a way of escape from the toil of thinking with precision. Far otherwise, the theory of chance has at least as great a need as any other one for setting forth its case in a punctiliously exact manner. Every required assumption ought to be explicitly formulated.

Now, the cardinal assumption is evident enough; it consists in supposing the values concerned to be analogous to those which arise from tossing a coin, throwing dice, cutting a pack of cards, or drawing tickets from a lottery urn. Further, in order that any theory should be entitled one of 'pure,' 'bare,' or 'simple' chance, such tossings, etc., must be taken to refer to the most fundamental quantities at issue. What, then, are the fundamental qualities involved in the measurements of abilities—or any other variables—and their correlations?

In answer to this question, we will follow the general practice of writing out the value of each ability or other variable in the following well-known form of Bravais,

$$X = X_1 e_1 + \dots + X_n e_n \qquad \text{......(1),}$$

where X is the total variable, the e's are elements varying independently from one individual to another, whilst $X_1, \dots X_n$ are coefficients of the elements and constant for all individuals though varying for different abilities. From the preceding equation we get

$$X - \bar{X} = X_1(e_1 - \bar{e}) + \dots + X_n(e_n - \bar{e}) \qquad \text{......(2),}$$

where the scoring indicates means.

On rewriting $X - \bar{X}$ as x and $e - \bar{e}$ as a, we get

$$x = X_1 a_1 + \dots + X_n a_n \qquad \text{......(3).}$$

Then the correlations between such variables will, as is well known, have the following form†,

$$r_{xy} = \frac{X_1 Y_1 + \dots + X_n Y_n}{(X_1^2 + \dots + X_n^2)^{\frac{1}{2}} (Y_1^2 + \dots + Y_n^2)^{\frac{1}{2}}} \qquad \text{......(4),}$$

or more simply,

$$= X_1' Y_1' + \dots + X_n' Y_n' \qquad \text{......(5),}$$

where X_p' denotes X_p divided by

$$(X_1^2 + \dots + X_n^2)^{\frac{1}{2}} \qquad \text{......(6).}$$

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* See *The Abilities of Man*, pp. 94–6.

† "Correlations of Sums and Differences," by present author, This *Journal*, v.

Here, then, are two kinds of values which may be assumed to vary by 'chance' or 'at random'; in the first place the elements or *e*'s (and therefore also the *a*'s); in the second place, the coefficients of these, such as $X_1, \dots X_n$, or alternatively $X_1', \dots X_n'$. But as regards the elements or *e*'s, a chance variation is postulated throughout the whole theory of correlation; it therefore cannot be what we are seeking; that is, an explanation of the special characteristics observed in such particular correlations as those between abilities. There remain, then, only the coefficients $X_1 \dots$ or $X_1' \dots$, to which the hypothesis of a purely chance origin of these special characteristics can legitimately be applied.

The further supposition may be made that the said coefficients take only the values of one or none. Then those elements whose coefficients happen to be one in any particular ability may be regarded as constituting a random 'sample' out of all the elements possible. At the same time, of course, they will furnish a sample of all the element-values possessed by any individual person. In this manner the sampling theory, although found quite invalid in respect of the mental elements discoverable by introspection, can now be revived for elements conceived hypothetically*.

II. THE CASE OF EXTREMELY NUMEROUS ELEMENTS.

Accordingly, the preceding ground was adopted by myself in the first definite investigation—as it seems—that was ever instituted into the doctrine of 'chance' as explaining the correlations between abilities. I brought forward "the not unplausible hypothesis, that each performance (*i.e.* each ability) depends on a randomly selected group of very numerous independent elements, and that the correlation between any two performances is due to some of the elements happening to be common to both groups†."

The hypothesis assumed that the coefficients X_1', $X_2', \dots X_n'$, had been obtained as randomly as if picked out of a lottery urn or drawn from a pack of cards. But in order to make the consequences more definite, the further assumption was added that the elements should be extremely numerous. This addition has not only entered into most of the fundamental work on chance, but is peculiarly important for the theory of ability; for here the elements are usually taken to represent such physiological entities as neurones of synapses, whose number runs into millions, or indeed into billions. And almost as extreme in this respect is the alternative interpretation of the elements as 'mental bonds.'

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* See *The Abilities of Man*, ch. v.

† *Psychol. Rev.* 1914, XXI.

Now, the chief consequence of these two assumptions was found to be that the inter-columnar correlation becomes equal to the correlation between the two abilities from which the two columns respectively derive. Or, as commonly written,

$$R_{r_{zp}.r_{zq}} = r_{pq}\text{*} \qquad \ldots\ldots(7),$$

where z takes all values except p and q. Such a result, of course, is quite unlike the inter-columnar correlations actually observed, since these tend to unity. No less incompatible is it with any satisfying of the criterion of tetrad differences.

Later on, I worked out the result of obtaining at random, not the $X_1', \ldots X_n'$, but the original $X_1, \ldots X_n$. This arrangement proved to have the advantage over the previous one that now the ensuing correlations do satisfy the criterion of tetrad differences[†]. But they do so in a strange manner; this is, by making all the correlations in the table equal to one another; so that here again there is no agreement with observation. And as for the inter-columnar correlations, these now become zero, and are therefore farther than ever from satisfying the criterion[‡].

Furthermore, whichever of the two sorts of coefficients we choose to select randomly (X_p or X_p'), we arrive at the untenable conclusion that every individual tends to have one and the same sum total of ability[§]. This and the preceding characteristics are obviously quite unlike those actually observed in respect of mental abilities. Indeed, they do not seem to agree with the observations made in any sphere of correlation, psychological or otherwise. Accordingly, the doctrine of chance or sampling along these lines had to be decisively rejected by me.

III. THE CASE OF MODERATELY NUMEROUS ELEMENTS.

The next step, naturally, was to drop the assumption that the elements are extremely numerous. For this purpose I made some actual trials with varying values of n, the number of elements. For convenience

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[*] *Psychol. Rev.* 1914, XXI. The proof is given in present Appendix I.

[†] But since thus the criterion is really satisfied, each measurement of any ability x, over and above being divisible into the extremely numerous e's, must necessarily *also* be divisible into the two factors 'g' and 's.' The connection between these two modes of division is given in the equation

$$'g' = S(X_p e_p)/n^{\frac{1}{2}}.$$

In this way the 'g' of any person would become a value representative of all his element-values. Such a fact would solely concern the interpretation of 'g,' not its actual measurement (still less its existence). The demonstration of all this has been furnished by Garnett, see this *Journal*, 1920, x, 242 ff.

[‡] For proof see Appendix II.

[§] For proof see Appendix III.

of calculation, the coefficients of the elements were made to take only the values of one or none; and similarly, with the values of the elements themselves. Further, I followed the usual practice of calculating the correlations for an infinite population, so that no corrections were needed for any errors of sampling.

The result was seen to indicate that with n as small as 4, every quantity becomes extraordinarily irregular. The correlations vary from perfect unity down to zero. Some of the 'g'-scores also go down to zero, and several of the tetrad differences are enormous. Whatever else such an arrangement may fit, it certainly has no likeness to what has been observed in the case of mental abilities.

On further increase of n, however, some very interesting results were obtained. For instance, the following table ensued from making $n = 40$.

Table of correlations with $n = 40$. Obtained by tossing as described. The correlations are arranged in best 'hierarchical' order.

	1	2	3	4	5	6	7	8	9	10
1		·63	·62	·62	·50	·54	·63	·62	·62	·35
2	·63		·69	·52	·49	·58	·53	·56	·51	·58
3	·62	·69		·64	·56	·47	·51	·59	·49	·49
4	·62	·52	·64		·56	·46	·47	·49	·54	·60
5	·50	·49	·56	·56		·71	·43	·45	·55	·50
6	·54	·58	·47	·46	·71		·43	·40	·55	·50
7	·63	·53	·51	·47	·43	·43		·65	·45	·40
8	·62	·56	·59	·49	·45	·40	·65		·31	·41
9	·62	·51	·49	·54	·55	·55	·45	·31		·30
10	·35	·58	·49	·60	·50	·50	·40	·41	·30	

Now, this table does present a remarkable likeness to many that have derived from various spheres of observation. And at first sight it may seem not very unlike some of the tables obtained in the sphere of abilities. Perhaps the correlations look more uniform than one might expect from an unselected set of abilities. But nothing very conclusive can be urged on this account. When we proceed, however, to apply the definite criteria of 'hierarchy,' the inter-columnar correlation turns out

Table of inter-columnar correlations obtained from the table preceding.

	1	2	3	4	5	6	7	8	9	10
1		·19	·44	– 32	– 12	–·29	·61	·39	·38	– 15
2	·19		·04	·57	·15	–·33	·36	·45	·05	–·38
3	·44	·04		·18	–·34	·08	·57	·52	·33	·49
4	–·32	·57	·18		–·08	·25	·03	·15	·24	–·10
5	–·12	·15	–·34	–·08		·34	–·57	–·49	·45	·35
6	–·29	–·33	·08	·25	·34		–·42	–·33	·45	·12
7	·61	·36	·57	·03	–·57	–·42		·84	·00	–·32
8	·39	·45	·52	·15	–·49	–·33	·84		·26	·12
9	·38	·05	·33	·24	·45	·45	·00	·26		·17
10	–·15	–·38	·49	–·10	·35	·12	–·32	·12	·17	

Average, +·12.

to average little over zero, whilst several of the tetrad differences—in spite of n being as much as 40—are very large indeed. On both counts, then, there is no agreement with what has been observed in the case of abilities.

'g'-scores obtained for 10 individuals: 21, 27, 15, 19, 19, 22, 22, 21, 21, 24.

An example of large tetrad differences: $r_{36} \cdot r_{25} - r_{23} \cdot r_{56} = \cdot 26$ with a P.E. of zero.

On still further increasing the magnitude of n, the correlations and the 'g'-scores tend to have very uniform values indeed, whilst nevertheless the tetrad differences still continue to be large. As for the intercolumnar correlations, these approach more closely than ever to zero. Once more, then, there is no agreement with what has been observed in the sphere of mental abilities. Throughout this sphere, then, we must conclude, the theory of 'pure,' 'bare,' 'mere,' or 'simple' chance is untenable.

IV. SPECIAL SYSTEMS INVOLVING CHANCE.

The preceding conclusion, however, by no means implies a rejection of all theories whatever wherein chance plays any part. On the contrary, as we have already seen, a chance variation at any rate of the e-values for different individuals is assumed in almost all correlational analysis. And with sufficient ingenuity further chance variation can be made to generate any result desired. For after all, the very essence of a system of 'chance' consists in postulating certain constant conditions and superposing on them certain variations which in the long run mutually cancel out. Thus in the end, the variations have had no influence at all; everything has instead depended on the constant conditions postulated. The only limit placed upon absolute arbitrariness lies in the fact that every set of postulated conditions will require its own justification.

On this account, we can no longer properly talk of a *general* theory of chance, but only of indefinitely numerous *special* theories involving it. A great contrast in this respect is afforded by the theory of 'two factors.' For this depends on the really general theorem, that hierarchy supplies the necessary and sufficient condition for analysing the variables into linear functions of 'g' and 's.' Here, we have perfect generality, since the coefficients of 'g' and 's' respectively may take any values whatever. No comparable general theorem has been even attempted in the case of chance or sampling.

Now, of these indefinitely numerous special systems of chance, only one appears to have so far received attention; it is that which we owe to Thomson. It was brought forward without noticing the earlier work that has been done on the 'simple' version chance*. Nor did Thomson himself introduce the concept of chance or sampling into his first relevant study. Instead, he then chose the coefficients of the elements quite arbitrarily, being at that time only bent on demonstrating that an inter-columnar correlation approximating to unity could possibly be obtained without any general factor being present†. In his later writings, however, he did introduce variation of a 'chance' nature. And the results not only differed from those which we have seen to flow from simple chance, but were totally opposed to these; for the inter-columnar correlations, instead of being approximately zero, became almost perfect unity; the tetrad differences tended correspondingly to vanish; and yet the correlations between the different abilities did not in the least tend towards one and the same value.

Let us, then, examine in detail the procedure by means of which such a complete transformation was effected. It fell into several separate steps. The first was for the purpose of settling the frequency of those elements in any ability which were to be shared by at least one of the other abilities, but not by all of them; such elements were called 'group factors'; the frequency of their occurrence was, of course, equivalent to the frequency with which their coefficients turned out to be other than zero. To determine this frequency, a draw was made from numbers ranging between 1 and 13 inclusive; the arrangement was such that each of these 13 numbers had an equal chance of being drawn. The second step was to identify each group factor with a particular die to represent it when throwing; for this, further draws were made, one for each of the factors. Then followed yet another phase; it was for the purpose of obtaining the elements in any ability which did not enter into any other ability; 'specific' factors these are called. This time again there was a draw for the frequency of the factors in each ability, every possible frequency being once more rendered equally probable; and then each such factor was taken to be represented by a particular die.

This very complicated scheme obviously introduced many assumptions that did not occur in the simple arrangement used in the earlier work. But many of these adjuncts were such as to make no significant difference in the eventual results. At least two of them, however, were of vital importance; in the first place, the assumption of 'two levels,' as indicated by separate drawings for the group factors and for the specific ones; secondly, the assumption that every different frequency will occur with equal probability.

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* This *Journal*, 1919, IX, 337–44.

† *Ibid.* 1916, VIII, 271–82.

Waiving the first of these two assumptions for the present, let us dwell a little upon the second of them. In every scheme of probability whatever, we may admit, some or other events are assumed to have equal probability. Witness the universally assumed equal probabilities of heads and tails when tossing a coin, of all the faces when throwing a die, or of all the cards when cutting a pack. So, too, we could quite plausibly take all the different elements as having an equal likelihood of occurring; that is, of having non-zero coefficients. This fits in well enough with the suggestion that has been made to interpret these elements as so many neurones, or so many synapses, or so many hereditary units.

But can we in similar fashion take all the different *frequencies of occurrence* as being equally probable? I venture to say that—with few, if any, exceptions—such an assumption does not appear in any of the standard works on the theory of probability, from Pascal and Fermat to Laplace, Venn, and Udny Yule. And with good reason, it would seem; for, in general, different frequencies of occurrence are *not* equally probable, or nearly so. Suppose that a bag contains a very large number of pennies, half being new and half old. Let 13 be drawn at random; the probability of getting, say, 6 or 7 new coins will not be equal to that of getting all 13 or of getting only one; instead, it will be nearly two thousand times greater. And just as much may be said if we replace the tossing of pennies by the throwing of dice, the cutting of cards, the drawing of tickets, etc.; in all ways alike, the different possible frequencies will have widely unequal probabilities. In fact, the whole theory of probability may be summed up as the ascertaining of how changes in frequency produce changes in probability. We seem here to be on bed rock; and any assumption disregarding it owes us at least some attempt at justification.

In any case, it should be noted, the said assumption is not merely additional to that of pure chance, it is in direct contradiction of it; for it implies that the coefficients X_1, $X_2, \dots X_n$, for any ability x are inter-correlated*. Consequently these do *not* constitute any genuine random 'sample.' Thus even when dealing with hypothetical elements, the attempt to explain abilities by sampling breaks down. And the same applies to all schemes whatever that engender results in conflict with those which we have found to flow from simple chance. In all such schemes alike the coefficients $X_1, \ldots X_n$ would necessarily be intercorrelated, so that they do not really vary by pure chance or constitute any genuine samples.

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* This is an immediate corollary of the theorem just mentioned, that when the coefficients are *not* inter-correlated, then the frequencies are *not* equal.

V. 'SANDY' AND 'BOULDERY' STRUCTURES.

Resuming the preceding pages, we first tried out the hypothesis of extremely numerous elements distributed among the different abilities, or other total variables, by 'chance.' On this hypothesis, each ability could quite properly be regarded as a 'sample' of the entire set of elements. And correspondingly, the values obtained by any individual for the elements in the ability could be regarded as a sample of the entire set of element-values possessed by him.

In addition to the elements being extremely numerous, it may be remarked, we tacitly made the further assumption that their respective influences or moments are all of the same order of magnitude. In fact, both Thomson and I have actually taken these moments to be equal. And such an addition seems to be legitimate enough. For if any finite proportion of the elements were of a lower order of magnitude than the others, they would not really be effective; that is to say, they would be negligible.

Of such a finely and evenly divided structure we find in nature copious examples; the grains of sand on the shore, the hairs on the head, the trees in a forest; or the leaves of the tree. Nor have plausible instances been lacking even in the domain of abilities. Thus, the suggestion has been made that the elements really represent the mental bonds, innate or acquired; or again, the neurones or synapses in the brain.

This hypothesis of extremely numerous elements was found by us to have four main consequences. All the correlations must tend to have one and the same magnitude. All the individuals must tend to have one and the same sum total of ability. All the inter-columnar correlations must tend to be zero. And all the tetrad differences must also tend to be zero. Only the last of these four consequences is in agreement with what has been actually observed in the case of abilities. Indeed, the first and second of them appear unlike what has hitherto been observed in any field of correlation whatever.

Moreover, this hypothesis is confronted by a host of further difficulties when compared with observation. For instance, there is the fact that the correlations between different abilities may be very large. In the much quoted work of Simpson, for example, they rose to as much as ·98. We should, on the present hypothesis, be committed to the strange position that some tests (given in a few minutes) involved at least 99 per cent. of all possible mental bonds or synapses, whilst other tests belonging to the same 'battery' of them involve only a minute fraction of these bonds or synapses!

Accordingly, this hypothesis of extremely numerous elements distributed by 'chance' and thus constituting a 'sample' had to be rejected.

We next proceeded to the alternative version of simple chance, namely as before except that the elements were taken to be only moderately numerous. Of such a structure also there are numerous physical illustrations. A shore, instead of being uniformly covered with fine sand, may exhibit varyingly sized stones and occasional comparatively huge boulders. The abandoning of the extremely numerous elements in favour of only a small number seems practically equivalent to renouncing the notion that the structure underlying ability has a sandy nature, and taking it instead to be bouldery.

But this greatly changes the psychological and physiological interpretations at our disposal. In particular the advocated mental bonds and cerebral neurones or synapses are at once thrown out of court. Their place would seem to be taken by such entities as the ductless glands, or perhaps some kind of hereditary units.

As regards the mathematical consequences of this bouldery view, they proved to be very erratic when the number of elements was as small as four; little likeness was exhibited to the correlations observed either in psychology or elsewhere. But when the number of elements was increased to forty, the consequences became much more interesting; for they were such as could be fairly matched in many fields of observation. But still they remained quite inapplicable to the correlations observed between abilities. There was indeed an improvement over the preceding version of chance (where the elements were extremely numerous) in that now the variation of the correlations and of the 'g'-scores became of fair size; but the inter-columnar correlations remained in the neighbourhood of zero; whilst the tetrad differences, which before did approach satisfactorily to zero, now departed greatly from this. On the whole, then, the theory of simple 'chance' or 'sampling' has in both its versions appeared inapplicable to the case of abilities.

In order to obviate all the preceding statistical difficulties, we have only to fall back on the doctrine of 'two factors.' Here we abandon the 'chance' nature of the coefficients X_1, $X_2, \dots X_n$, and instead suppose that for each ability only two of these coefficients take values other than zero, one of these two supplying an element specific to that ability whilst the other gives an element common to all abilities.

But however satisfactory this solution may be from the purely statistical standpoint, there remains a possible objection as regards its psycho-physiological interpretation; for the plea may be urged that we actually know of *several* different influences on ability; as for example the above-mentioned various ductless glands. How, it may be asked, can such a plurality of influences and the unity of '*g*' ever be mutually reconciled?

An answer is supplied in the fact that, if such multiple influences really occur, they certainly do so under some limiting condition. Otherwise, it would be possible for two abilities p_1 and p_2 to be favoured specially by, say, the pituitary secretion, whilst two others a_1 and a_2 were favoured by the adrenal secretions; and then the tetrad difference

$$r_{p_1 p_2} \cdot r_{a_1 a_2} - r_{p_1 a_1} \cdot r_{p_2 a_2}$$

could not possibly be zero, as agreement with observation demands. Hence the condition which we need must be such as to prevent the *p*'s from favouring other abilities than the *a*'s do; this means that the *p*'s and the *a*'s must somehow be *equivalent and interchangeable.* Such an interchangeability was in fact, as shown by Garnett, really introduced by the assumption of the elements being very numerous and their coefficients distributed according to fixed probabilities; it was the essential reason why in such a case the tetrad differences tended to vanish. But such a multitudinousness has had to be abandoned by us, being untenable on other grounds. And to make plausible any interchangeability when the elements cease to be multitudinous is quite another thing.

A solution to the problem is afforded by remembering that a similar one has been encountered and mastered in the case of physical science. This had to make intelligible the observed fact that movement in one place is interchangeable with movement in another. And it did so by inventing the hypothesis that all the movements are manifestations of one and the same underlying general energy; see the admirable account of Nunn*. In our present problem we can make a kindred hypothesis, by taking all the elements as being contributory to one and the same underlying psychical or psycho-physical energy. On so doing, our dilemma disappears; the tetrad differences may be reduced to zero without raising any difficulties in other respects. And it is interesting to note that here—for the first time—a reason for regarding '*g*' as a measurement of some sort of energy is discovered in the very nature of the '*g*'-scores and the ensuing correlations. Always previously, the energetic hypothesis had been prompted not by individual but solely by general psychology.

* *Proc. Arist. Soc.* 1912, pp. 25–64.

APPENDIX I.

The inter-columnar correlation on the hypothesis of independent relative chances.

Using (5), the columns of correlations derived from any two abilities a and b may be written as follows:

$$\begin{array}{lll} & a & b \\ c & A_1C_1 + \ldots + A_nC_n & B_1C_1 + \ldots + B_nC_n \\ d & A_1D_1 + \ldots + A_nD_n & B_1D_1 + \ldots + B_nD_n \\ \cdot & \cdot \quad \cdot \quad \cdot & \cdot \quad \cdot \quad \cdot \\ \cdot & \cdot \quad \cdot \quad \cdot & \cdot \quad \cdot \quad \cdot \end{array}$$

Then the correlation between the columns under a and b respectively can be expressed in terms of correlations between the binary elements as follows ("Correlations of Sums and Differences," see footnote on p. 250):

$$R_{r_{ax}r_{bx}} = \frac{S\,(AB\sigma_X{}^2) + SS\,(AB\sigma_X\sigma_X r_{XX})}{\sqrt{S\,(A^2\sigma_X{}^2) + SS\,(AA\sigma_X\sigma_X r_{XX})}\,\sqrt{S\,(B^2\sigma_X{}^2) + SS\,(BB\sigma_X\sigma_X r_{XX})}},$$

X being the variable whose particular values are C, D, etc.

Let us now, in accordance with our hypothesis, suppose that all the coefficients A, B, C, D, etc., have been drawn independently of each other so that $r_{XX} = 0$. Thereupon we at once get

$$R_{r_{ax}r_{bx}} = \frac{S\,(AB\sigma^2)}{\sqrt{S\,(A^2\sigma^2)}\,\sqrt{S\,(B^2\sigma^2)}}\,.$$

But since $\sigma_X{}^2$ does not involve any A's or B's, the preceding equation may be transformed into

$$R_{r_{ax}r_{bx}} = \frac{\bar{\sigma}^2 S\,(AB)}{\bar{\sigma}^2\,\sqrt{S\,(A^2)}\,\sqrt{S\,(B^2)}} = r_{ab}.$$

Q.E.D.

APPENDIX II.

The inter-columnar correlation on the hypothesis of independent absolute chances.

This time we have to use (4) instead of (5), and the proof is correspondingly more difficult.

Let us write $X_m \sqrt{S\,(X^2)}$ as F_m. Expanding this by Taylor's theorem, we get

$$F_m = (F_m)_0 + \left(\frac{\partial F_m}{\partial X}\right)_0 dX + S\left(\frac{\partial F_m}{\partial X}\right)_0 dX + R,$$

where dX represents $X_m - X_0$, and in all cases the zero index indicates the value obtained on replacing each X by the mean of all the coefficients. R contains the terms which involve higher differentials.

Hence, by actual differentiation,

$$dF = \frac{1}{m^{\frac{1}{2}} X_0}\, dX - \frac{1}{m^{\frac{3}{2}} X_0}\, S\,(dX),$$

neglecting higher powers of the differentials.

From this, with some calculation in which I have the pleasure of acknowledging assistance from Prof. Filon, there ensues

$$R_{r_{ax} r_{bx}} = 0.$$

Q.E.D.

APPENDIX III.

Equal total ability when n is very large and the e's are positive.

Denoting the sum total of all the e's for any individual by T we get

$$T = S\,(e) = n\bar{e}$$

and

$$\sigma_T{}^2 = S\,(\sigma_e{}^2) = n\sigma_e{}^2,$$

so that $\sigma_T/T = n^{\frac{1}{2}}\sigma_e/n\bar{e}$, which tends to zero.

Hence, if we write the total ability of any individual as T_x, the mean of all the T's as $\bar{T}$, and the difference between these two as d_T, there ensues $T_x = \bar{T} + d_x$, where d is negligible in comparison with the quantity to which it is added, so that sensibly $T_x = \bar{T}$. That is to say, every individual has a T of one and the same magnitude.

(*Manuscript received* 31 *August* 1927.)

PERSISTENCE: A CHARACTERISTIC OF REMEMBERING

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In the course of a series of experiments which were being carried out for another purpose one of us (C.) in 1923, working on an ergometer with a load of 1 kilogram at a comfortable rate, noted that the rate quite involuntarily adopted over many experiments was between 108 and 110 revolutions per minute. A year and ten months later, on repeating the same type of experiments, he found the rate unconsciously adopted lay between 110 and 118 revolutions per minute. When the other (D.) was tested in the same fashion, his rate in 1923 lay between 128 and 130 revolutions, and two years later, it was 130. Similar experiments were made on other subjects with like results. There seemed, therefore, to be at least the possibility that each subject had a 'natural' rate and accordingly an experimental investigation was undertaken with the idea of proving or disproving this hypothesis.

What do we mean exactly when we speak of a natural rate, what does the term 'natural' connote? Obviously it is meant to refer to a rate of performance which is 'implanted, existing or present by nature,' a rate which is in other words 'inherent in the very constitution of a person,' a rate which is innate and which has neither been acquired or assumed as the result of previous practice nor adopted after watching other, perhaps nominally skilled, performers carry out the same or similar series of movements. But are we ever justified in assuming that such innate proclivities exist, and if they exist is there any physiological basis for their existence? At first sight it would seem to be a matter of common knowledge that the physical conformation of the individual determines the normal rate of performance of his daily activities: the long-legged man tends to take fewer and longer steps in unit time than does his short-legged fellow.

As it is commonly used 'natural' often means 'comfortable,' *i.e.* the rate at which the work can be done in the easiest and least exhausting manner to the performer. It also sometimes means the rate which gives the greatest output with minimum effort, and the rate which is maintained with least variation. These meanings are not, of course, mutually exclusive, indeed, if one's rate of working be largely determined by innate conditions, it will probably be the most comfortable and the most usual. The primary meaning, however, is undoubtedly unlearned, inherent in the very constitution of the person.

It is, however, exceedingly difficult to use this concept with exactness. How can one decide whether a particular action or group of actions is innate? If we had only to take into consideration the physical structure of the subject, there might be some hope of an adequate answer, but emotion, interest, the whole psychical environment during the performance of even a reflex form of activity plays an important rôle in governing the rate of performance. There is the attitude of the "whining schoolboy creeping like a snail unwillingly to school," his house of bondage, in contrast to the brisk and active movements which normally characterize the healthy schoolboy in pursuit of pleasures of his own devising. Or again a man walks to and from his work very comfortably at the rate of 120 steps per minute. Is this his natural rate, and how can we know that it is natural? How can we be sure that it is the expression of some inborn characteristic and not a relic of his military training? The question becomes even more complex when it is recognized that under other conditions, say, going to church, the rate is different. Does one's natural pace vary with the object one has in view or with the mood of the moment? Is there, in fact, a natural rate for each set of circumstances? The difficulty may be accentuated by asking the converse question, "How can we know whether an activity has been learned?" or "What are we to take as evidence of learning from experience?"

It is commonly assumed that modification of behaviour to meet new situations is evidence of learning from experience, but this criterion can be applied only with great difficulty for three reasons: (1) Because some habits are formed so very slowly that their formation escapes attention. Most of the trivial habits of everyday life belong to this category. (2) As reactions tend to repeat themselves in the same way when the same stimulus is presented, the chance occurrence of any action tends to form the basis of a habit. (3) Innate activities do not all appear at birth, some of them emerge comparatively late in life when the animal is approaching maturity.

The question whether we each have our own natural rate of working might be investigated by looking for some physical character which varies concomitantly with the rate of working. Although physical configuration and the psychological 'set' of the individual play a part and admittedly an important part in determining the rate of performance, there is definite evidence of an active perhaps rudimentary factor governing periodicity in the organism.

It might also be investigated by training different subjects to work at different rates and finding whether, when left perfectly free to select their own rate of movement, they adopt the rates at which they have been trained. In the event of their unconsciously adopting this rate we have a certain amount of presumptive evidence against the view that a natural rate exists. We should here be assuming, of course, that the impulse to work at the 'innate' rate is stronger than the tendency to work at any other rate, no matter how much time has been spent in learning it. In doing so we might indeed be assuming too much, for it is always possible that the 'natural' rate might be preferred only in the absence of well-formed and deeply ingrained habits.

In view of the ambiguity of the word 'natural' and the difficulty of its elucidation, it seems to us better for the present, at any rate, to substitute for the question "Have we each our own natural rate of performance?" other questions which can be stated more precisely and are capable of being attacked experimentally, questions such as: (1) Does the same worker tend to work at the same rate under similar conditions? (2) Is this rate maintained under varying conditions? (3) If it varies, what is the effect of specified conditions, objective and subjective? (4) Is this rate, supposing it to be constant, correlated with any physical or mental characters?

In the course of the early experiments, which naturally in the first instance were purely tentative, we were not long in discovering that if there was a 'natural' rate it could be influenced by a large number of factors, the varying importance of which we have found it difficult to disentangle. We therefore determined to attempt to isolate and study some of these conditions before proceeding with our original enquiry.

The aim of this communication is to report one of, what we take to be, the most fundamental of the interfering phenomena, which we have ventured to call 'persistence.'

The ergometer used in our experiments is described in the *Journal of Physiology*, LVIII, 1923, p 92. It consists of a solid metal wheel, about the size of a bicycle-wheel, which can be rotated by alternately pushing and pulling two levers, the push and pull being in a horizontal direction. Over·the rim of the wheel passes a belt the tension of which can be varied so as to change the load against which the work is done. The magnitude of the load can be read from a balance which is permanently attached to the belt. Near the axle of the wheel is a metal wipe contact by means of which and certain electrical contrivances the rate of rotation of the wheel is recorded. To keep the wheel rotating a periodic to-and-fro movement is necessary. In order not to distract the subject, the recording apparatus was put into a different room from the ergometer.

Working with this instrument we found that, when the subject was directed to adopt a comfortable rate he struck one which was remarkably constant from day to day, but when it followed a period during which he had been working at a higher or lower rate, it always tended toward this latter rate. Consider a typical instance. D. had been working at his own rate (65 rotations per half-minute) for 11 minutes, then, after keeping time for 12 minutes to the ticking of a metronome, marking time at the rate of 42 wheel rotations each half-minute, he was instructed, the metronome having been stopped, to resume his natural rate; his own rate then fell to 57 rotations per half-minute. The previous low rate had, then, pulled the 'natural' rate down 8 rotations per half-minute. On the other hand, when the repetition of the 'natural' rate was preceded by a high one, the displacement was upwards.

The records of all these preliminary experiments are summarized in Table I, where in the first column is given the 'natural' rate, in the second column are the means of the immediately preceding 'distracting' rates, and in the third are the means of the reproductions of the 'natural' rates following them. The figures in brackets are the probable errors of the means. Readings were taken every alternate half-minute and the means shown in this table and throughout the whole paper are means per half-minute.

TABLE I.

Subject	'Natural' rate	Distracting rate	Subsequent 'natural' rate
C.	55	92·0 (0 5)	73·0 (0·5)
,,	,,	77·7 (1·1)	68 3 (0·2)
,,	,,	72 0 (0)	60·7 (0·8)
,,	,,	45·0 (0)	52·2 (0·5)
,,	,,	31·6 (0·2)	42·0 (0·2)
D.	65	96·2 (0·5)	73 8 (0·7)
,,	,,	93 7 (1·3)	73·7 (0·4)
,,	,,	88·0 (0)	69·9 (1·3)
,,	,,	37·6 (0·4)	56·9 (0·4)
,,	,,	34·6 (0·8)	53·0 (0·3)
,,	,,	34·4 (0·5)	61·1 (0·6)*

Both subjects show the same tendency. When the preceding 'distracting' rate is higher than the normal, the reproduction of the latter is higher than usual, and when it is lower, the normal is lower than usual.

The reliability of these results is enhanced by the fact that they came to our notice only after the experiments had been performed. We were concerned to find with what degree of exactness we returned to the 'natural' rate after various disturbing circumstances. The influence of the previous rate of working asserted itself only in a review of the whole of the experiments.

As the normal rate is evidently subject to variation, we tried to gain control over the conditions by working with other rates and adopting a slightly different procedure. The subject worked for 3 to 8 minutes at a moderate rate, then for 3 minutes he rested, after which he struck a rate appreciably higher or lower and tried to keep it constant for about 6 minutes. Then followed another 3 or more minutes' rest, after which he attempted for 5 to 8 minutes to reproduce the rate at which he worked during the first period. The rest-periods were introduced partly to give the subject an opportunity of getting his mind off the work and so making the subsequent recollection more truly an effort of memory, and partly to prevent the onset of fatigue: in both respects we appeared to be quite successful. The means of the rates per half-minute of the three spells of work are shown in Table II. The probable errors of these means are given in brackets.

TABLE II.

Subject	First rate	Second rate	Reproduction of first
D.	77·8 (0·2)	96·8 (0·5)	79·4 (0·3). After 20 mins. rest.
,,	65·3 (0·9)	95·2 (0·4)	73·7 (0·4). No rest.
,,	68·4 (0·3)	84·4 (0·4)	72·9 (0·3). After 3 mins. rest.
,,	45·3 (0·3)	70·1 (0·4)	53·5 (0·5). ,, ,,
,,	54·8 (0·5)	69·7 (0·2)	59·9 (0·3). ,, ,,
,,	69·0 (0·9)	41·6 (0·8)	61·4 (0·5). No rest.
C.	52·3 (0·6)	82·7 (1·2)	55·9 (0·2). After 20 mins. rest.
,,	68·3 (0·2)	40·5 (0·5)	54·5 (0·5). After 3 mins. rest.

The outcome of these observations is the same as that of the first series, *i.e.* according as the second rate is distinctly above or distinctly below the first, the reproduction is above or below the original. In this series, which was tentative, the procedure in the different experiments varied slightly. In two sets of experiments no rest-periods were introduced, while in two others an interval of 20 minutes was allowed between the second and third spells of work. In every case the persistence effect appears, although in the 20-minute rest experiment it seems to be distinctly less marked.

The experiments so far described were performed solely by and with ourselves: they were purely exploratory in character. In order to test our generalizations we made further observations with other subjects, adhering carefully to the procedure described above. The subject was instructed to strike a moderate, comfortable rate and to sustain it. He was told that after a time he would be asked to reproduce it and that the object of the experiment was to find with what degree of accuracy one could reproduce a given rate after working at a different one. He was, however, kept in complete ignorance of the results of previous experiments. This form of instruction had, we think, the effect of putting the subjects on their mettle. They were told to attend only to the rate, to get the same muscular 'set' as in the first series, and not to speculate on the possible effects of the distraction. We believe that this instruction was carried out successfully. After working at the comfortable rate for 6 to 8 minutes, the subject rested for 3 minutes, then worked at a self-selected high (or low) rate for 5 to 8 minutes, rested 3 minutes, then reproduced to the best of his ability the first rate. Sometimes this was followed by another rest of 3 minutes and a 6-minute reproduction of the second (distracting) rate. The subjects were all skilled observers and were either members of or workers in the Institute of Physiology (Glasgow University). The results of these observations are summarized in Table III.

TABLE III.

Subject	(1)	(2)	(3)	(4)
G.	55·1 (0·3)	88·2 (0·5)	67·7 (0·5)	99·0 (0·2)
,,	57·4 (0·4)	43·8 (0·4)	52·8 (0·5)	40·8 (0·2)
,,	54·5 (0·8)	33·9 (0·9)	41·4 (0·7)	13 mins. rest between (2) and (3)
B.	64·2 (0·4)	40·7 (0·1)	59·7 (0·3)	35·1 (0·2)
,,	58·5 (0·3)	73·4 (0·8)	63·5 (0·7)	74·4 (0·2)
T.	62·6 (0·5)	41·8 (0·3)	53·4 (0·3)	38·8 (0·4)
,,	53·3 (0·2)	70·3 (0·5)	62·7 (0·3)	79·5 (0·8)
McC.	74·7 (0·2)	95·3 (0·7)	76·2 (0·3)	
Bu.	43·6 (0·3)	67·9 (0·1)	45·0 (0·7)	
W.	51·7 (0·1)	68·5 (0·3)	53·3 (0·2)	
,,	33·8 (0·6)	70·2 (0·7)	38·4 (0·1)	
,,	38·2 (0·5)	17·8 (0·9)	33·4 (0·3)	
Ga.	37·6 (0·1)	66·4 (0·1)	41·7 (0·3)	
,,	44·8 (0·2)	29·6 (0·8)	41·2 (0·5)	
,,	39·2 (0·1)	62·8 (0·7)	44·8 (0·8)	
,,	37·0 (0·2)	66·6 (0·9)	41·0 (0·4)	No rest between (2) and (3)
,,	42·0 (0·3)	63·0 (1·0)	47·0 (0·4)	20 mins. rest between (2) and (3)
M.	70·2 (0·4)	80·4 (0·5)	76·6 (0·2)	
,,	67·8 (0·5)	49·2 (0·8)	63·2 (0·9)	
,,	49·3 (0·5)	34·3 (0·5)	39·2 (1·1)	
We.	35·7 (0·3)	56·0 (0·7)	38·0 (0·2)	20 mins. rest between (2) and (3)
,,	32·8 (0·2)	47·3 (0·3)	37·4 (0·3)	9 mins. rest between (2) and (3)

In every case there is a displacement towards the preceding rate: even the reproduced high rate is higher than at first, and the reproduced low rate lower. There are some suggestions that the amount of the displacement varies from person to person and is fairly constant for the same person, but the initial and distracting rates were so different that it is difficult to make a comparison. Much more extensive experiments will be required, as it will be necessary to determine the limits of individual variability from day to day, before one can compare the variability of one subject with that of another.

It seems reasonable to infer from these tables that our estimate of rate of movement has to a large extent reference to immediately preceding rates and that in reproducing a set of movements there is a tendency for their rate to approximate to that of immediately preceding movements of the same kind. It is as if there were in the psycho-physical organism a kind of inertia, a general persistence of attitude or 'set,' corresponding to physical inertia. This 'carry-over' is what we have ventured to call 'Persistence.' The term is not altogether satisfactory because it has in popular speech implications not present in our use of it: but it is the best we could find.

Several questions suggest themselves here, such as (1) how long does this effect last? (2) does the amount of displacement vary with the amount of the difference between the first and the second rates? (3) does it vary with the duration of the distracting work? To the first of these it is not easy to give a definite answer, for much depends on what has happened between the two periods of work. The human organism is so perfectly self-regulating, it responds so readily to variations in its environment, that it seems to be comparatively easy voluntarily to shake off, at any rate for a few minutes, the influence of the previous rate, *e.g.* by stretching oneself or by making a spurt or working at any very different rate. Apart from these variations, which are not necessarily exceptions to the rule, the effect seems to be fairly lasting.

In Tables II and III are some observations made with a rest of 13 to 20 minutes between the 'distracting' spell and the reproduction. They indicate that persistence had not disappeared after that interval. More importance is to be attached to the records in Table III, for they were quite incognitive, *i.e.* the subjects had no knowledge of the results of previous experiments. Those in Table II were made with ourselves as subjects after we had discovered the persistence phenomenon and so are less reliable. Further work on this question is needed both to determine the duration of the after-effect and to find how it varies with time.

To the second question only a tentative answer can be given. In Table I are shown the means of the 'natural' rates with the distracting rates which preceded them. With one exception, marked by a star, the amount of displacement varies roughly with the difference between the two rates. The exception is only apparent, for, although the mean of the preceding rates was 34, they varied from 32 to 39, the last three being 39, 38, 35, and it was probably these later periods that proved most effective in influencing subsequent rates. Table III provides no information on this question, for it would be misleading to compare the records of one subject with those of another, and there are not sufficient observations on the same subject under similar initial conditions to make any other procedure possible. It seems, then, as if the amount of the influence exerted by the preceding rate does vary with the difference between that rate and the next; but further observations, made under carefully controlled conditions, are needed.

As regards the third of our questions, we cannot give even a tentative answer to it.

The experiments so far described lasted only an hour or two: we have, however, performed one which was spread over a fortnight. The subject (Ga.) was instructed to select his own rate, a comfortable one, and work at it for half an hour. The next two days (Tuesday and Wednesday) he repeated this pace for half an hour each day. Then for four days (Thursday, Friday, Saturday and Monday) he worked for 30 minutes to the ticking of a metronome at a rate which was sensibly higher than that of the first three days. On the following day (Tuesday) he reproduced the metronome rate from memory, and on the next three days he reproduced the rate of the first three days. The self-selected rate was continued through three days in order to find how accurately he could reproduce this rate when there was no distraction; for the same reason the metronome rate was repeated from memory on Tuesday of the second week. The means of the rates on the several days were:

1. Monday: self-selected rate 38 6.
2. Tuesday: self-selected rate 31·3.
3. Wednesday: self-selected rate 31·3.
4, 5, 6, 7. Thursday, Friday, Saturday, Monday: working to metronome at 45.
8. Tuesday: reproduction of metronome rate 39·0.
9. Wednesday: reproduction of original rate 33 9.
10. Thursday: reproduction of original rate 29·8.
11. Friday: reproduction of original rate 28·9.

The most striking thing about these figures is their gradual fall. The tendency to reduce the speed may be due to the fact that this subject was working at a low level. In other experiments he usually selected a rate varying from 50 to 55 rotations per half-minute. It is probable that the prospect of half an hour's steady work caused him to select—unwittingly no doubt—a rate which there was some hope of maintaining without undue effort. One effect of working below one's capacity (as we hope to show in a later paper) is a tendency to work more and more slowly, a tendency which appears very clearly here.

If we compare the work of the ninth day with that of the third, which under the circumstances seems to be the most reasonable procedure (for the rates of the second and third days represent the subject's reproduction of that of the first without intervening distraction), then the persistence effect appears here, but on subsequent days it is obscured by the constant tendency to fall.

It should be remarked that we have worked with a very limited range of conditions, the initial rate being generally a moderate one, and the distracting rate very appreciably different. It still remains to be found whether the phenomenon appears when the initial rate is very high or very low and when the distracting rate is only slightly different from the other.

The procedure has been so far as possible, indeed must be, incognitive. The subject must not know the results of similar experiments with other subjects, and it is advisable that he should not speculate on the probable effect of the disturbing work, for his speculations will certainly affect his work. If he knows what results have been given by others, his temperament will largely determine his own: obstinate, critical, contra-suggestible people then give effects contrary to that described above, while agreeable, suggestible people oblige by increasing the amount of the carry-over. It is for this reason that we have discarded the results of the experiments in which subjects had been told the result of previous experiments.

Introspection has suggested that the mere reproduction of a specified rate of movement is not such a simple affair as it appears at first sight, for one may adopt any one of several attitudes, (1) one may simply aim at striking a rate which will evoke the same muscular experiences as the original, or (2) one may aim at striking a high or a low rate (the first having been high or low), *i.e.* one which is merely very different from the distracting rate, or (3) one may have in mind the interval between the second and the first, and try to reproduce it. These different attitudes presumably have different effects and, as they are usually adopted non-voluntarily, and rarely reported, it is by no means easy to separate them and their effects. In the observations just reported we tried to encourage the adoption of the first attitude.

Some additional observations have shown that the persistence phenomenon appears in the reproduction of hand and finger movements as well as in the bigger arm movements. Further, it does not seem to be confined to speed, for it appears in the reproduction of a particular strength of grip, though not so clearly. In a series of experiments with the hand-grip dynamometer (Smedley's) the subjects were instructed to grip the instrument several times in succession and always with the same force. They did not see the dial and were not told the amount of the grip. After a rest of 2 or 3 minutes they chose a grip appreciably stronger or weaker and repeated it 5 to 10 times: then after another rest they repeated the first series. The means are shown in Table IV. With two exceptions the reproductions in the third series are deviated towards the 'distracting' grip.

TABLE IV.

Subject	(1)	(2)	(3)
S.	27·7	22·7	25·6
,,	14·1	29·9	18·8
F.	18·7	12·7	18·4
O.	29·1	19·4	25·4
,,	18·2	33·5	27·2
Sc.	26·5	32·2	26·3
T.	26·4	32·2	27·9
,,	15·7	33·1	18·2
,,	25·1	15·2	29·7
C.	9·2	20 9	10·3
D.	15·2	28·4	19·8
,,	11·4	33·1	14 5
O.	16·4	32·1	19·7

The reproduction of finger and hand movements was investigated in two ways, (1) by means of a lever attached at its end to the first finger and moving about its centre, and (2) by means of a reaction-key (Morse key) connected in circuit with a battery and an armature operating a pointer the movements of which were recorded on a kymograph. The movement was a simple finger or hand movement; with the lever apparatus it was usually a finger movement, with the reaction-key it was generally a hand and finger movement. The following table gives a typical set of readings:

TABLE V. *Showing number of taps made in* 5 *seconds.*

Subject	First rate	Distracting rate	Reproduction of first
R.	14	24	15
,,	13	7	10
B.	9	5	7
,,	7	3	6
S.	11	23	15
,,	23	15	21
,,	12	7	10

Persistence is clearly present here.

The last of these subjects (S.) is a skilled musician with a reputation as a composer and performer. He shows the persistence phenomenon just as clearly as the other less skilful performers. Compare his records with those of subjects who are accustomed to playing jazz music, where the aim apparently is to achieve metronome regularity, and an interesting difference appears: the reproductions of the latter subjects are on the whole more accurate, although their endeavour to keep strict time shows itself in greater irregularity in the lengths of the individual beats. Here are three specimen records:

Subject F.

(1) Self-selected rate	first quarter-minute	16·5	strokes	in	10 seconds
	second ,,	19	,,		,,
(2) Higher rate	first ,,	27	,,		,,
	second ,,	30	,,		,,
(3) Reproduction of (1)	first ,,	16·5	,,		,,
	second ,,	18	,,		,,

Subject M.

(1) Self-selected moderate rate	12·5	strokes in	10 seconds
(2) Faster rate	46	,,	,,
(3) Reproduction of (1)	12·25	,,	,,

Subject M.

(1) Self-selected moderate rate	18	strokes in	10 seconds
(2) Slower rate	6	,,	,,
(3) Reproduction of (1)	19	,,	,,

The accuracy of reproduction here is remarkable; in fact, in the records of subject M., who is a teacher of dancing and gymnastics, there is a suggestion of an attempt to correct the persistence effect: the subject herself, however, was not conscious of any such effort, she merely knew that she kept good time (probably by checking her movements against the beating of a metronome): the correction had probably been learned without ever having been made explicit. It seems, then, that training in keeping strict time does result in the control and apparent elimination of persistence.

It is an obvious criticism of these results that they were reached under somewhat abnormal conditions: the movements were extraordinarily simple and nowhere, except perhaps in a modern dance orchestra, does one try, by a series of unvaried tappings, to attain metronome regularity. It is, therefore, reasonable to ask, "Does persistence appear when the conditions approach more nearly to the normal activities of everyday life?" What happens, for instance, when a pianist varies the tempo of his playing? This question is easily answered. We opened a piano and laid a thin copper wire over the hammers, and above this placed a bar of metal so that every time a key was struck the wire and metal were brought into contact. By substituting this wire and bar for the terminals of our reaction-key we were able to record the actual movements made by a pianist playing normally on an apparently normal key-board.

The piano and the recording apparatus were installed in different rooms and connected by twin wires. The metal contacts produced a faint metallic sound every time the keys were struck. We were afraid at first that this might disturb the sensitive souls of our musical subjects, but were interested to find that, although they showed some uneasiness when the keys were first struck, they quickly became adapted to it and it had no appreciable effect on their playing: one of them who had been left alone in the piano room so far lost himself in his aesthetic rhapsody that he failed to hear our crude signals, a loud hammering on the wall separating the two rooms.

The results of our experiments with this instrument corroborated those obtained with the simple reaction-key. Subject S., a skilled musician, played with his right hand an Andante by Mozart, then a study of his own composition which moved at a much quicker pace, then he reproduced the Andante. The time taken to play the same phrase of the Andante (measured by the tracing made on the smoked paper) was 24·6 seconds in the first performance and 21·8 seconds after the quick melody, *i.e.* the reproduction was 12·8 per cent. quicker than the original.

Another subject, who was also a skilful pianist, began by playing with his right hand a piece of Mendelssohn which went at a moderate pace, this was followed by a quick piece of Chopin, then came two reproductions of the Mendelssohn, next a slow piece from Handel, and finally another reproduction of the Mendelssohn. The time taken to play the selected phrase was:

(1) Mendelssohn, moderate rate	23·3	seconds
(2) Chopin, quick		
(3) Reproduction of (1)	21·0	,,
(4) ,, ,,	19·7	,,
(5) Handel, slow		
(6) Reproduction of (1)	23·1	,,
(7) ,, ,,	23·2	,,
(8) ,, of (5)		
(9) ,, of (1)	26·3	,,

The effects of both the quick and the slow performances are quite clear.

The same subject played at certain prescribed rates another passage from Chopin, which is normally played at a moderately rapid rate, with the following result:

(1)	Chopin,	normal moderate	18·5 seconds
(2)	„	quickly	8·3 „
(3)	„	like (1)	15·9 „
(4)	„	„	16·3 „
(5)	„	slow	39·0 „
(6)	„	like (1)	20·3 „

Another subject whose playing was of a very different kind, mainly dance music, (subject M.), played a Scottish reel containing a fast, a slow, a fast, a moderately fast, and a fast movement, in which all 'fasts' were alike in tempo, with the following results:

	Both hands	Both hands	One hand
(1) Fast	20 in 5·5 seconds	12 in 3·3 seconds	12 in 3 seconds
(2) Slow			
(3) Fast	19 „	11·7 „	11 „
(4) Mod. fast			
(5) Fast	20 „	11·5 „	10·5 „

The figures show the number of movements made in the time given.

The persistence here is very slight except in the last performance: a result much like that obtained with the same subject in the reaction-key experiments. This subject also played a slow melody, then a quick one, then the first again, with the following results:

	Both hands	Right hand
(1) Slow	11 in 4 seconds	8·5 in 3 seconds
(2) Quick		
(3) Like (1)	10·5 „	9·5 „

The reproduction was almost exact when both hands were used, in fact, there is a suggestion of contra-persistence: a slight persistence effect appears in the one-hand experiment.

In every respect, then, these experiments with the piano confirm the results given with the simpler reaction-key: persistence appears in the normal finger movements of a musician and it appears less clearly or is absent from the playing of those who have given much attention to playing in strict time.

As a matter of interest it should be remarked that there is a most striking irregularity in the timing of the individual notes and that this irregularity appears to be much greater in the playing of the 'strict-time' artists than in that of those who play for pure aesthetic enjoyment.

This well-marked difference between musical performers suggests a speculation on what is called personality in music. Artists generally view with suspicion the use of measuring devices and the adoption of a scientific attitude towards their art: the use of a metronome tends, they say, to make their playing mechanical, to take the soul out of it. The results of our experiments suggest that this 'soul' is, at any rate partly, the expression of natural laws of mental behaviour, laws of which we are vaguely conscious but which we have not analysed. What the musician calls 'feeling' must express itself in variations of pitch, intensity, tempo and other attributes of sounds, which are natural expressions of thought and feeling and just because they are natural excite a resonance in the minds of listeners. His playing must faithfully express his thoughts and feelings: it must be true to mind itself. Hence the very clear persistence effect in it as he moves from one tempo to another. Each modifies the next in this subtle but analysable way, and any attempt to eliminate this modification by mechanical control (for it is only by means of mechanical controls that we can get away from the workings of our own minds) results in making the performance seem unnatural, stiff, machine-like, soul-less. In other words the artist's playing appeals because it has those qualities whereby it is recognized as the normal expression of thought and feeling.

SUMMARY.

1. The rate at which simple repetitive movements are reproduced is deflected toward the rate at which intervening movements of a similar kind have been made.
2. This persistence tendency shows itself not only under laboratory conditions, where the movements are simple, uniform and periodic, but also under the more complex conditions of piano playing.
3. It can be controlled, perhaps eliminated, by training.
4. It is readily obscured or exaggerated by consciousness of its presence.

NOTE. An alternative name for the phenomenon which we have provisionally called 'persistence' is 'diabatic' from Gr. διαβατικός = able to pass through. We have to thank Prof. W. Rennie for suggesting the work.

(*Manuscript received* 2 *October* 1927.)

LA CAUSALITÉ CHEZ L'ENFANT[1]

PAR J. PIAGET.

IL n'y a que trois manières d'interpréter la naissance de la notion de cause, si nous laissons provisoirement de côté l'interprétation sociologique à laquelle nous reviendrons dans la suite. Ou bien la notion de cause provient de l'expérience externe, c.-à-d. des associations imposées par les choses elles-mêmes; ou bien la notion de cause provient de l'expérience interne, c.-à-d. du sentiment de l'activité personnelle; ou bien la notion de cause est une relation que la raison établit entre les choses, ou entre les choses et le moi, et qui résulte ainsi simplement de notre capacité de déduction. La première de ces trois hypothèses a été soutenue par Hume: la causalité, pour lui, provient des habitudes que nous acquérons, sous la pression des choses. La seconde hypothèse a été soutenue par Maine de Biran: la causalité est née de la volonté et de l'effort musculaire. La troisième hypothèse est celle des grands rationalistes en général.

Nous n'allons pas faire ici de la philosophie, mais de la psychologie de l'enfant. Nous ne discuterons donc pas ces hypothèses en elles-mêmes, c.-à-d. que nous ne nous demanderons pas où elles conduisent en théorie de la connaissance. Nous allons simplement rechercher ce qu'elles valent comme instruments de travail dans l'analyse de l'esprit de l'enfant. Or, chose curieuse, elles sont très fécondes toutes les trois.

Quand nous examinons le petit enfant, celui de 2 ans à 7 ans, nous avons constamment l'impression que Hume et Maine de Biran ont raison tous les deux, quoiqu'ils se contredisent, et quand nous examinons l'évolution toute entière de la causalité, jusqu'à 12 ou 15 ans, nous avons l'impression que la notion de cause est un instrument de connaissance que la raison modifie et reconstruit sans cesse. Analysons rapidement cette situation paradoxale, et nous trouverons ainsi l'hypothèse de travail qui nous conduira ensuite dans la description des faits.

Lorsqu'on observe la première enfance, on a sans cesse la conviction que Hume a raison: c'est d'une manière toute empirique et toute phénoméniste que le petit enfant établit les relations de cause à effet. Il constate ainsi qu'on allume le soir les phares des bicyclettes et des automobiles, et il conclut que c'est la lanterne qui fait avancer la bicyclette. On lui demande alors comment cela se peut, et comment une lanterne fait tourner des roues. Il n'en sait rien et ne s'en soucie nullement. Pour lui, comme dit Hume, "n'importe quoi peut produire n'importe quoi." Seulement, et en même temps, le même petit enfant se conduit d'une manière très curieuse. Il croit que la lanterne a de la force. Il croit qu'elle est vivante, qu'elle fait exprès de faire avancer la bicyclette. Il lui dit: "gentille lanterne" ou "méchante lanterne." Bref, il la conçoit comme une sorte de personne. Or, cela, nous ne le comprenons plus du tout, si la causalité est née seulement de l'expérience toute empirique et de l'habitude. En effet, avant toute expérience, l'enfant perçoit déjà les choses d'une certaine manière. Il projette ses sentiments sur l'univers entier. Il ne distingue pas le subjectif de l'objectif: il est beaucoup moins près des choses que nous-mêmes.

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[1] Conférence donnée à Cambridge le 4 Mars 1927, sous les auspices de la Cambridge Education Society.

Donc Maine de Biran a aussi raison. Dès le berceau, l'enfant fait l'expérience de ses sensations musculaires, de ses états de plaisir et de peine, de ses désirs et de ses déceptions. Or, il ne peut pas découvrir le monde extérieur, c.-à-d. faire l'expérience de la résistance des choses, sans concevoir les choses en corrélation avec son moi. C'est de là qu'est née la notion de force, l'animisme, bref toutes les formes élémentaires de la causalité, qui sont beaucoup plus près de la réalité psychologique que de la réalité physique objectivement conçue. Seulement, ici encore, de graves difficultés surgissent. D'une part, l'enfant ne conçoit son moi (sa pensée, etc.) que par analogie avec ce qu'il observe dans le monde extérieur. Et surtout, d'autre part, le petit enfant n'a qu'une conscience très relative de son moi. Il découvre les choses bien avant de découvrir sa propre personne, et il découvre la personne des autres avant la sienne. Il faut donc dire qu'il confond sans cesse le subjectif et l'objectif, mais il est faux de dire qu'il découvre *d'abord* son moi et qu'il conçoit *ensuite* les choses par analogie avec ce moi.

Nous pouvons donc, en ce qui concerne les premières années de l'enfant, faire une hypothèse de travail que nous chercherons à vérifier tout à l'heure. Nous constatons que Hume et Maine de Biran se réfutent l'un par l'autre. Hume a raison de dire que la causalité chez l'enfant commence par le tâtonnement empirique, mais il n'explique pas que l'enfant conçoive les choses en fonction de son moi. Maine de Biran a raison de dire qu'il y a des éléments subjectifs dans la causalité primitive, mais il n'explique pas pourquoi l'enfant s'occupe des choses bien avant de découvrir son moi. Cela étant, prenons simplement acte de ce paradoxe et supposons ce qui suit. Admettons qu'aux débuts de la vie mentale, il n'y ait aucune limite entre le moi et le monde extérieur.

Cela revient à dire que la conscience du nouveau-né ne saurait connaître les choses en elles-mêmes mais que, dans toute connaissance primitive, les éléments provenant des choses et les éléments provenant du corps sont indissolublement unis pour la conscience. Autrement dit, encore, connaître, pour le bébé, c'est assimiler les choses à des schémas dessinés par l'action organique, de telle sorte que, pour la conscience, les choses paraissent affectées de qualités qui proviennent en fait de l'organisme. Si nous nous en tenons à ces suppositions, il semble que nous allons pouvoir comprendre toute l'évolution de la causalité chez l'enfant. En effet, deux conséquences essentielles se déduisent de ce point de départ, et deux conséquences dont l'observation nous montrera le bien fondé.

Première conséquence: le petit enfant doit être à la fois plus près et plus loin des choses que nous-mêmes. En effet, l'enfant sera plus près des choses que nous, en ce sens que, sa pensée n'ayant pas encore pris conscience d'elle-même (et cela puisqu'elle procède par tâtonnement empirique et non encore par déduction), il ne saura pas se libérer de l'apparence immédiate pour construire une réalité rationnelle plus profonde. Mais il sera plus loin des choses que nous, en ce sens qu'il mêlera sans cesse à ses conceptions des éléments subjectifs dont nous sommes débarrassés. On pourrait dire aussi bien, d'ailleurs, que le petit enfant est à la fois plus près et plus loin de son moi que nous-mêmes: plus près puisqu'il le mêle à tout, et plus loin puisqu'il n'en prend pas conscience. C'est en ce sens que Hume et Maine de Biran ont raison tous les deux...et se réfutent l'un par l'autre.

Seconde conséquence: en se développant, l'enfant se libérera à la fois de l'apparence empirique des choses et des adhérences subjectives de sa pensée. Ainsi, pour prendre un exemple, il renoncera à croire que le soleil et la lune le suivent dans ses promenades, et ne verra plus là qu'une simple illusion. Mais, en même temps, il renoncera à croire que les astres sont vivants et conscients comme nous. En effet, en détachant son moi des choses, pour ainsi dire, il sera obligé de corriger les apparences: il ne pourra plus croire que la lune le suit, s'il découvre qu'elle paraît suivre en même temps Pierre, Jacques et Jean, et il sera bien forcé de la considérer comme immobile ou comme avançant indépendamment de nous. Mais, en détachant sa pensée des choses, l'enfant cessera également de concevoir les choses à l'image de son moi. Il cessera de croire à la vie et à la conscience des choses, ainsi qu'aux forces anthropomorphiques. Or, s'il en est ainsi, la causalité, envisagée dans son dynamisme, et non plus seulement dans ses origines, est un produit de la raison: en détachant son moi des choses, c.-à-d. en se détachant à la fois de l'apparence empirique et des illusions subjectives, l'enfant est obligé de construire peu à peu une réalité plus profonde que celle de l'expérience immédiate, et c'est en cette construction que consiste toute la causalité.

Cela dit, passons à l'analyse des faits. Nous allons maintenant chercher à décrire le plus objectivement possible les stades de la causalité chez l'enfant, et à vérifier le bien fondé des hypothèses de travail que nous venons d'établir. Mais, comme la causalité touche à presque toutes les manifestations de la pensée de l'enfant, nous devrons nous limiter à une seule classe d'explications, celle de *l'explication des mouvements*, quitte à faire, à l'occasion, quelqu'incursion dans les domaines connexes.

Nous allons ainsi demander aux enfants comment avancent les nuages, les astres, les rivières, le vent, ce que sont les vagues ou les courants d'air, pourquoi les nuages restent en l'air et les bateaux sur l'eau, tandis que les cailloux tombent sur le sol ou au fond de l'eau, etc. En classant nos réponses, nous constaterons l'existence de quatre grands stades dans la causalité enfantine.

Durant un premier stade, dont on constate de nombreuses manifestations jusque vers 3–4 ans, et qui laissent quelques traces dans la suite, l'explication est *à la fois magique et phénoméniste*: les choses sont liées à certains gestes que nous exécutons, et cela sans aucune liaison spatiale ou intelligible.

Durant un second stade, qui s'étend environ de 3 à 8 ans, la cause des mouvements de la nature est *morale*, et, en analysant cette forme de causalité, nous trouverons un mélange d'animisme et d'artificialisme. Cette causalité morale n'exclut d'ailleurs pas la connaissance de certaines liaisons physiques, mais le physique et le moral sont encore confondus.

Durant un troisième stade, qui s'étend de 8 à 11 ans environ, la cause des mouvements devient principalement physique, mais la physique de l'enfant de cet âge est imprégnée de *dynamisme*: les corps sont mus par des forces substantielles, comme dans la physique d'Aristote, et l'enfant n'a encore qu'une idée très vague du mécanisme proprement dit, avec ses principes rationnels de conservation.

Enfin, vers 10–11 ans apparaît une *explication mécanique* du mouvement, ainsi que certains principes rationnels comme la conservation du poids.

Comme on le voit, cette progression génétique paraît d'emblée s'accorder avec ce que nous disions tout à l'heure. D'une part, la causalité semble se désubjectiver progressivement: d'abord magique, puis morale, elle devient simplement dynamique, puis enfin mécanique. Mais, en même temps, la construction rationnelle semble remplacer progressivement l'association empirique: au phénoménisme succède l'explication par un ordre moral caché, puis par des forces que l'enfant cherche à rendre intelligibles, puis enfin par des principes logiques que l'expérience suggère simplement, sans les dicter, et que la raison seule construit.

I. Le stade magico-phénoméniste primitif

Dans quelques jours, j'aurai l'honneur d'analyser devant la Société britannique de Psychologie, la causalité chez le bébé de quelques mois[1]. Je chercherai à montrer que, lorsque le bébé perçoit une liaison entre un de ses gestes et un mouvement du monde extérieur, il attribue à ce geste une efficace et s'en sert pour reproduire le mouvement perçu auparavant. Ainsi un bébé clignera des yeux devant le cordon d'une lampe électrique pour faire allumer la lampe, ou fera un geste de la main pour faire remuer un objet à distance. Il semble ainsi que la causalité primitive soit à la fois phénoméniste et quasi-magique, et cela parce que le bébé ne fait aucune distinction entre son moi et le monde extérieur et considère ainsi ses efforts musculaires comme se prolongeant dans les choses elles-mêmes.

Or, chose curieuse, nous trouvons de nombreux restes de cette attitude primitive jusque chez l'enfant de 3 et de 4 ans. Nous n'en analyserons que deux.

Il y a tout d'abord les explications les plus primitives du mouvement des astres et des nuages. L'enfant découvre très tôt que, lorsqu'il marche, les astres et souvent les nuages paraissent le suivre. Il y a là une liaison qui est imposée par l'expérience, et qui, comme telle, est de nature phénoméniste, dans ses racines tout au moins. Seulement, en présence d'une telle observation, on peut prendre au moins trois attitudes possibles, et ce sont ces attitudes qui sont intéressantes au point de vue de la causalité, car elles montrent très clairement l'orientation d'esprit de l'enfant.

L'une de ces attitudes est l'attitude critique. Nous constatons que les arbres ou les murs paraissent se déplacer quand nous marchons à côté d'eux et que les paysages s'enfuient à toute vitesse quand nous sommes en train. Or l'enfant, qui est d'abord dupe de ces apparences, s'en libère rapidement. En ce qui concerne les astres, il pourra donc se demander ce que fera la lune si lui-même part dans une direction et son frère dans la direction inverse. Il aboutira ainsi à considérer les mouvements des astres qui nous suivent comme de pures apparences.

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[1] Cette conférence a paru ici même, vol. XVIII, p. 97.

Une seconde attitude possible est l'attitude animiste. Si le soleil et la lune nous suivent, c'est qu'ils désirent nous suivre. Ils s'occupent de nous, nous regardent, nous surveillent, nous attendent quand nous entrons dans une boutique, etc. Mais, s'ils le voulaient, ils pourraient s'en aller. S'ils nous suivent, c'est donc qu'ils le veulent bien.

Une troisième attitude possible est l'attitude magique, ou attitude de la croyance en l'efficace personnelle. Si les astres nous suivent, c'est que nous les forçons à nous suivre. Ils nous obéissent. C'est nous qui les faisons avancer, ou s'arrêter, et qui réglons leur vitesse. Ils ne peuvent pas nous désobéir.

La première de ces trois attitudes est assurément la plus difficile, puisqu'elle suppose la réflexion critique. Aussi est-il bien naturel qu'elle se présente la dernière en date. Ce n'est, en effet, que vers 8 ans, en moyenne, qu'elle devient générale (chez les $\frac{3}{4}$ des enfants), c.-à-d. durant notre troisième stade, ou stade du dynamisme.

Mais, notons-le, les deuxième et troisième de ces trois attitudes paraissent logiquement de difficulté égale. Rien, dans l'expérience, ne nous indique si la lune nous suit parce qu'elle le veut bien, ou si elle nous suit parce que nous l'y forçons, puisque, en fait, elle nous suit toujours. Il est donc intéressant de nous demander laquelle de ces deux orientations d'esprit est la plus primitive. Or, là-dessus, les résultats de la statistique sont très nets. L'enfant commence par croire à son efficace personnelle, et par penser qu'il force les astres à lui obéir. L'attitude animiste ne vient qu'en second.

Chronologiquement, les réponses témoignant d'une croyance en l'efficace personnelle sont donc caractéristiques de notre premier stade, et sont surtout fréquentes vers 4 ans. Les réponses suivant lesquelles les astres nous suivent parce qu'ils le veulent bien sont surtout fréquentes entre 5 et 8 ans, et sont caractéristiques de la causalité morale, comme nous le verrons dans la suite.

Passons à un second exemple, qui nous servira en même temps de contre-épreuve. Pour contrôler notre analyse des croyances précédentes, nous avons imaginé un dispositif présentant à l'enfant une convergence entre un de ses gestes et un mouvement extérieur. L'expérience est comparable, par conséquent, au cas des astres qui paraissent nous suivre. Nous montrons à l'enfant une pipette pleine d'eau, en retenant l'eau dans la pipette, ce qui naturellement excite au plus haut point l'intérêt du sujet. Puis nous disons: "Quand tu voudras que l'eau tombe tu feras un petit signe avec le doigt." L'enfant remue le doigt, et, sans qu'il voie comment nous faisons, nous laissons tomber un peu de l'eau contenue dans la pipette. L'enfant recommence l'expérience, nous laissons de nouveau échapper quelques gouttes, et ainsi de suite.

Ici, comme tout à l'heure, trois orientations d'esprit sont possibles en présence de ce mystère. L'enfant peut prendre une attitude critique et dire: "Je n'y comprends rien, mais il y a certainement un truc. C'est vous et pas moi qui faites tomber l'eau." L'enfant peut prendre une attitude simplement phénoméniste, et dire: "Il y a une relation entre le mouvement de mon doigt et la chute de l'eau, mais je n'y suis personnellement pour rien. Sans doute, mon doigt ébranle l'air, ou il se passe quelque chose d'analogue." Ou bien, enfin, l'enfant peut prendre l'attitude de la croyance en l'efficace personnelle, et dire: "C'est moi qui fais tomber cette eau, parce que je la force de tomber et qu'elle m'obéit."

Or, chose curieuse, c'est de nouveau la croyance en l'efficace qui est la plus primitive. Les petits, de 3 et 4 ans sont d'emblée convaincus qu'ils ont un pouvoir sur l'eau de la pipette. Si c'est, par exemple, un garçon qui fait l'expérience, et qu'une étudiante, ensuite, fasse le signe du doigt sans succès, le garçon dit sans plus: "Elle sait pas y faire. Ça va pas aux filles, aux demoiselles. Ça va rien qu'aux garçons...," etc. Vers 7 ans, au contraire, l'enfant estime qu'il y a un truc caché et que c'est l'expérimentateur qui fait tomber l'eau. Entre deux, on assiste à une diminution progressive de la croyance en l'efficace. On voit donc que ces résultats sont bien parallèles à ceux de la question de la marche des astres.

Bref, il semble que chez le bébé, de 6–8 mois à 2 ans environ, la causalité primitive se présente sous la forme d'une sorte de croyance à l'efficace du geste, croyance favorisée par les succès obtenus au cours d'expériences tout empiriques ou phénoménistes. Il semble, en outre, que cette forme de causalité réapparaisse toutes les fois que l'occasion en est offerte à l'enfant, et cela jusqu'à une date qu'il est difficile de préciser, quoique ces résidus de la causalité primitive soient très fréquents jusque vers 4 ans.

Disons encore, avant de quitter ce premier stade, que, à la causalité magico-phénoméniste, correspond une représentation des choses qui rappelle à beaucoup d'égards les fameuses "participations" si bien décrites par M. Lévy-Bruhl dans la mentalité des peuples dits primitifs. Je ne veux pas aborder ici la question de la genèse de ces participations chez le bébé, question qui est très complexe car elle touche à toute la constitution de la notion d'objet. Notons seulement les explications que les enfants de 4 ans donnent encore de l'apparition et de la disparition de certains phénomènes spécialement difficiles à concevoir comme des choses, c.-à-d. comme des êtres permanents et identiques à eux-mêmes. Il s'agit de l'explication des courants d'air et des ombres.

Prenons un éventail et agitons-le. Demandons à l'enfant comment s'est formé le courant d'air qu'il constate ainsi. Ayons soin, en outre, de faire l'expérience dans une chambre dont la fenêtre a été fermée avant l'arrivée de l'enfant. A partir de 6 et 7 ans, l'enfant donne des explications qui ne nous intéressent pas ici, et qui reviennent à dire que l'éventail a créé de l'air par son mouvement, car l'enfant ne sait pas qu'il y a de l'air immobile dans la chambre. Mais les petits, de 4 et même souvent encore de 5 ans, nous donnent une interprétation beaucoup plus curieuse. Ils ignorent naturellement, et *a fortiori*, que la chambre est pleine d'air; et alors, ils estiment que l'éventail, par son mouvement, a provoqué l'apparition d'un courant d'air, lequel se trouve provenir en même temps du vent qui souffle en dehors de la fenêtre. En effet, l'enfant, sentant le courant, dit aussitôt: "C'est du vent," et lorsqu'on lui demande d'où vient ce vent, il affirme qu'il vient "de dehors." On fait remarquer au petit que la fenêtre est fermée, mais cela ne le gêne pas. Il continue à soutenir que le vent de l'éventail n'est autre que le vent du dehors, qui accourt dans l'éventail au moment où on l'agite. Quand l'éventail s'arrête, le vent part, quand on recommence, il revient.

Il en est de même pour l'ombre. L'ombre est naturellement une substance, pour l'enfant, et non une absence de lumière. Mais c'est aussi une substance qui, comme l'air, a le pouvoir de se déplacer instantanément. Quand on fait de l'ombre sur la table, avec un livre, l'enfant de 4 ans reconnaît bien que c'est le livre qui fait l'ombre, mais en même temps il affirme que l'ombre vient de dehors: il y a beaucoup d'ombre sous les arbres, il y en a aussi au ciel, dans les nuages qui produisent la nuit, et alors, quand on met un livre sur la table, cette ombre des arbres ou du ciel vient se précipiter là. Elle repart quand on soulève le livre, et revient quand on le pose à nouveau !

Il est clair que ces croyances bizarres proviennent simplement de ce que l'enfant établit une identité là où nous ne faisons qu'une comparaison. Au lieu de dire: "Cette ombre du livre est analogue à celle des arbres," ou "Ce courant d'air est analogue au vent," il dit: "Cette ombre c'est celle des arbres," et "Ce courant d'air, c'est le vent." Mais il est bien difficile de ne pas voir dans ces réponses un stade antérieur à nos identifications proprement logiques, puisqu'ici l'identité s'établit sans aucun souci du contact spatial: l'ombre, qui était dans le ciel à l'instant, est maintenant sous le livre, puis retourne au ciel, etc. On pourrait presque soutenir que la chose est à plusieurs places à la fois, s'il ne valait pas mieux dire simplement que l'enfant établit une liaison sans s'occuper en rien des connexions géométriques. Il y a donc là quelque chose de comparable à la participation. Or, comme on le voit d'emblée, de telles représentations sont le prolongement tout naturel de la causalité magico-phénoméniste décrite à l'instant, et dont le caractère essentiel est précisément de n'être pas spatiale.

Cela dit, passons au deuxième stade de la causalité.

II. LE STADE DE L'EXPLICATION MORALE

Si notre analyse du premier stade de l'explication chez l'enfant est exacte, nous avons vérifié une partie de notre hypothèse de travail: la causalité débute par le maximum d'empirisme phénoméniste joint au maximum de dynamisme subjectiviste. L'enfant qui croit faire avancer le soleil est, en effet, plus près que nous de l'expérience directe des choses, mais en même temps il attribue à son efficace personnelle des pouvoirs plus grands qu'il ne le fera dans la suite. A la limite, le bébé projette donc son moi sur l'univers entier tout en présentant la plus grande docilité aux suggestions de l'expérience immédiate.

Que deviendra cette situation durant le second stade de la causalité chez l'enfant, c.-à-d. en moyenne de 3 à 7–8 ans? L'enfant continuera, d'une part, à suivre très docilement l'apparence des choses, quoique cependant un peu moins que durant le premier stade. Ainsi, il continuera à croire que les astres le suivent, mais il le croira moins facilement des nuages. Il continuera à mettre les astres et les nuages sur le même plan spatial, mais il les situera cependant plus haut dans le ciel que précédemment. Il cessera de croire qu'il lui suffit de fermer les yeux pour produire la nuit, mais il pensera encore que si tout le monde allait se coucher au milieu de la journée il ferait sans doute aussitôt noir. Bref, il restera très phénoméniste tout en l'étant moins que précédemment. Quant à l'aspect subjectif des liaisons causales, la croyance en l'efficace va se muer en une liaison simplement morale.

En effet, mieux l'enfant apprend à distinguer son moi de celui des autres et moins il attribue d'efficace à ses propres gestes. En apprenant à imiter autrui, et ensuite, grâce au langage, en apprenant à obéir à ses parents, l'enfant fait une expérience essentielle qui va colorer d'un nouveau jour toute sa représentation du monde. Il va concevoir l'univers comme une vaste société d'êtres vivants soumis à un ensemble de devoirs et de contraintes, et la causalité deviendra ainsi une simple liaison de coercition, mi-physique et mi-morale, analogue à la contrainte que les adultes exercent sur les enfants.

Pour vérifier cela, questionnons les enfants de 7 à 8 ans sur le mouvement du vent, des nuages, des astres et des rivières. Nous trouvons ainsi un résultat très net et confirmant entièrement ce que le regretté Prof. James Sully avait esquissé dans ses fameuses *Études sur l'Enfance*: c'est que ces corps sont conçus comme de grands enfants sages, qui accomplissent correctement leur devoir.

A vrai dire, nous trouvons, durant ce stade de l'explication morale, deux types différents de réponses qui caractérisent deux sous-stades successifs. Durant un premier sous-stade, qui s'étend de 3 à 6 ans principalement, l'enfant se représente les choses comme des êtres vivants obéissant directement à l'homme (ou à Dieu, ce qui revient au même puisqu'ils définissent Dieu comme un "Monsieur"). Durant le second sous-stade (de 6 à 8 ans), les choses sont subordonnées les unes aux autres, et l'ensemble de la nature est subordonné à des règles morales, qui sont bien encore anthropocentriques, mais dans lesquelles l'artificialisme explicite et mythologique du début du stade n'apparaît plus directement. Au point de vue de la causalité auquel nous nous plaçons ici, la structure de l'explication est donc le même durant les deux sous-stades.

Commençons par le premier, qui est donc caractérisé par un mélange d'animisme et d'artificialisme, et demandons aux enfants de 4–6 ans comment se forme le vent et pourquoi l'air avance. Les réponses sont toujours les mêmes. Le vent "*c'est quelqu'un qui a soufflé.* — Qui, ça? *Des hommes.* — Quels hommes? — *Des hommes qui faisaient ce métier*" (6 ans), ou "*c'est quand on siffle; ça sort de l'air*," ou c'est le Bon Dieu et les hommes qui font de l'air avec une pompe à bicyclette. Quant à savoir pourquoi l'air avance, la cause en est, d'une part, physique, comme on vient de le voir, mais elle est surtout morale: le vent souffle "*pour casser les arbres pour faire du feu*" (6 ans), ou encore "*pour faire beaucoup de vagues et puis ça pousse les petits bateaux et on n'a pas besoin de ramer*" (6 ans), ou encore "pour apporter les nuages, pour faire pleuvoir," etc. Le vent a surtout un ensemble de fonctions dans l'économie des mouvements de la nature, comme nous le verrons dans la suite.

Demandons maintenant à l'enfant comment avancent les nuages. Nous trouvons ici, comme à propos du vent, d'ailleurs, des séries de transition entre l'explication par l'efficace personnelle et l'explication morale et artificialiste. Ainsi, ma fille, à vingt mois déjà, disait en voyant les nuages: "*Brouillard fumée papa,*" exprimant par là qu'elle assimilait les nuages à la fumée de la pipe de son père. Dans ces conditions il va de soi que le mouvement des nuages s'expliquera très tôt par l'action de l'adulte. Ainsi, un petit garçon de 3 ans me disait des nuages: "*C'est le mécanicien qui les fait marcher,*" parce qu'il avait vu la fumée sortir de la cheminée des locomotives. D'autres feront intervenir des ouvriers, des maçons, ou le Bon Dieu, et mêleront à ces explications, plus ou moins, suivant les cas, l'hypothèse que nous faisons avancer les nuages en marchant ou qu'ils nous suivent dans nos courses.

Mais, ici encore, quel que soit le moyen physique invoqué, la cause du mouvement devient très vite morale. Les nuages avancent non pas au hasard, mais parce qu'ils ont des séries de rôles à remplir. Ils avancent parce qu'ils doivent faire ou annoncer la pluie. Ils doivent dans certains cas pousser le soleil et la lune, soutenir le ciel, etc. Ils ont surtout une fonction très imprévue pour l'intelligence adulte, mais à laquelle l'enfant songe très souvent: ils font la nuit. En effet, pour l'enfant comme pour les premiers penseurs de la Grèce jusqu'à Empédocle, la nuit est une substance, une sorte de vapeur noire qui remplit tout l'atmosphère. D'ailleurs, comme nous l'avons vu, il en est de même de l'ombre, et les enfants croient jusque vers 9 ans, que les objets font de l'ombre même la nuit, seulement qu'on ne la voit pas précisément parce qu'il fait nuit! Or, cette substance noire constituant la nuit est précisément conçue comme issue des nuages, ou comme un grand nuage foncé. Par conséquent, l'une des fonctions des nuages est de faire la nuit, aussi, les enfants à qui l'on demande à brûle pourpoint pourquoi avancent les nuages répondent-ils souvent: "Pour faire la nuit," ou plus simplement "A cause de la nuit."

Le mouvement des astres est expliqué d'une manière très analogue. D'une part, ce sont les hommes ou Dieu qui les font avancer. D'autre part, s'ils avancent, c'est pour nous chauffer, nous éclairer, nous surveiller, nous conduire, etc. L'étoile guidant les Rois Mages vers le berceau de Jésus est évidemment un reste de ce stade de l'explication du mouvement.

Le mouvement de l'eau des rivières, enfin, est expliqué par une causalité toute semblable. D'une part, ce sont les bateaux, les rames, etc., qui font avancer la rivière. Mais, d'autre part, l'eau de la rivière est consciente et vivante: elle coule pour faire avancer les bateaux et les nageurs; elle coule aussi pour aller dans la mer, pour nous donner de l'eau, etc.

Bref, dans toutes ces explications primitives du second stade, on observe le même mélange d'artificialisme et d'animisme. L'explication est artificialiste en ce sens que ce sont les hommes qui constituent la cause première et la fin dernière de toute activité naturelle. Mais cet artificialisme suppose l'animisme, en ce sens que, pour servir les hommes, les choses doivent être vivantes, conscientes de leur devoir, et assez intelligentes pour l'accomplir. C'est ce que nous voulons dire en prétendant que la causalité de ce stade est essentiellement morale.

Si nous passons maintenant au deuxième sous-stade, c.-à-d. aux explication des enfants de 6 à 8 ans en moyenne, nous trouvons un type tout à fait analogue de causalité, mais transposé sur la nature elle-même. Autrement dit, ce ne sont plus les hommes qui sont cause du mouvement des choses, mais les choses elles-mêmes. Au premier abord cela paraît très différent, puisque l'explication semble être devenue purement physique. Seulement, dès qu'on analyse d'un peu près ces réponses on s'aperçoit que la nature est toujours conçue comme une société d'êtres vivants soumis à des règles, et qu'ainsi la forme seule de l'explication a changé, le fond restant en fait toujours essentiellement moral et anthropocentrique.

Voici des exemples. Lorsqu'on demande à l'enfant comment avancent les nuages, il répond que c'est la nuit, ou la pluie, ou le froid et le mauvais temps, ou le soleil et la lune, qui font avancer les nuages, ou enfin qu'ils avancent tout seuls. Tout cela paraît au premier abord étrangement fantaisiste et hétérogène. En réalité, toutes les explications reviennent à dire que les nuages doivent avancer pour exercer leur fonction propre par rapport à l'homme. Dire que la nuit est cause du mouvement des nuages, c'est de nouveau affirmer simplement que les nuages doivent avancer pour faire la nuit. Dire que la pluie ou le froid font courir les nuages, c'est dire que les nuages doivent venir nous apporter ou nous annoncer la pluie. Dire que le soleil et la lune sont cause de la marche des mêmes nuages, c'est dire que le soleil chasse les nuages comme un gendarme chasse un vagabond par sa vue seule, ou que la lune accompagne les nuages dans leur course vers la nuit. Bref, au fond de tout cela est une confusion fondamentale entre la loi physique et la loi morale ou sociale.

De même, la marche des astres est due aux nuages, à la pluie, à la nuit, au vent, etc., parce que les astres, étant des êtres vivants et conscients, qui ont des devoirs précis à accomplir, fuient la pluie pour porter ailleurs la chaleur ou viennent de nuit nous éclairer, etc.

L'explication du mouvement des rivières présente quelques particularités curieuses. Une réponse qui revient souvent, par exemple, est que l'eau des rivières avance à cause des cailloux. En fait, cette explication a de nouveau un sens à la fois physique et moral. D'une part, le caillou étant lourd, pèse sur l'eau et produit un courant, mais surtout, d'autre part, le caillou fait avancer l'eau parce qu'il lui sert d'obstacle: en présence du caillou l'eau "prend son élan" pour passer par-dessus, comme un cheval devant une haie, et c'est cet élan qui crée le courant. Or ce courant, dit toujours l'enfant, est nécessaire parce que l'eau "doit couler," pour conduire les bateaux, pour nous donner à boire, etc.

La flottaison des bateaux ou la suspension des nuages est le prétexte au même mélange déconcertant d'empirisme et de moralisme. Tel enfant affirmera tout à la fois que les petits bateaux flottent parce qu'ils sont légers et les grands bateaux parce qu'ils sont lourds. De même, tel enfant dira que les astres restent en l'air parce qu'ils sont légers et les nuages parce qu'ils sont lourds. L'enfant justifiera cela en disant que les objets légers sont portés par l'eau ou par l'air, et que les objets lourds ont la force de se porter tout seuls. Mais le fond de l'affaire est de nouveau que les règles déterminant ainsi la flottaison des corps ont un aspect aussi moral que physique. Les nuages "doivent" rester en l'air, parce que sans cela ils nous tueraient dans leur chute. Quant à savoir pourquoi les cailloux coulent au fond de l'eau et pourquoi les bateaux flottent, un garçon de 6 ans nous a dit, et cela est très représentatif de ce stade: c'est parce que les bateaux sont "plus intelligents" que les cailloux, c.-à-d. qu'"ils ne font pas ce qu'il faut pas faire" !

Bref, jusque vers 7–8 ans, l'explication du mouvement, comme d'ailleurs la causalité en général, témoigne d'une perpétuelle assimilation du physique et du moral. Les mouvements ont ou n'ont pas, suivant les cas, de causes proprement matérielles. Mais, lorsqu'ils en ont, le mécanisme physique du mouvement n'est qu'un instrument au service de la vraie cause, laquelle est une cause morale: cette vraie cause, c'est l'obligation où sont les choses d'accomplir leur fonction, conformément à un plan harmonieux dont l'homme est à la fois l'auteur et la raison d'être.

III. LE STADE DE L'EXPLICATION DYNAMISTE

Jusque vers 7–8 ans, les causes physiques du mouvement ne sont ainsi ni indispensables ni suffisantes. Assurément, l'enfant dit déjà parfois que c'est le vent qui fait avancer les nuages ou les astres, ou les rivières. Mais le vent n'est qu'un moyen utilisé par ces corps, comme l'oiseau peut utiliser la brise dans son vol. Sans cet adjuvant, leur mouvement serait le même, et cette influence accessoire du vent ne suffit en rien à expliquer la cause profonde des mouvements.

A partir de 7–8 ans, au contraire, tout mouvement est conçu comme résultant de mécanismes physiques indispensables à sa production. En outre, quoique les corps ou certains corps continuent d'être considérés comme vivants et conscients, la cause physique du mouvement apparaît de plus en plus à l'enfant comme suffisant à l'explication de ce mouvement. Seulement, comme nous allons le voir, cette cause physique est encore conçue, de 7–8 à 10–11 ans environ, d'une manière très particulière.

En effet, il n'est pas possible que l'animisme enfantin, qui est si vivace jusque vers 7 et 8 ans, disparaisse entièrement en une fois. Nous constatons, au contraire, que les corps conçus par l'enfant comme doués d'un mouvement propre, par opposition à ceux dont le mouvement est acquis de dehors, continuent à être considérés comme vivants et conscients. Or nous allons voir justement que la plupart des êtres naturels paraissent à l'enfant jouir de la capacité de se mouvoir en partie seuls, au rebours des machines, qui sont mues par l'homme. D'autre part, même lorsque l'enfant refuse à un corps la conscience et la vie, il lui conserve une propriété qui ressemble étrangement à une vie sans conscience, et qui n'est autre que la *force*, conçue comme une force substantielle inhérente aux corps eux-mêmes.

Dans ces deux cas, celui où les corps continuent à être tenus pour vivants et celui où ils sont simplement doués de forces substantielles—et ces deux cas se confondent presque l'un avec l'autre—les corps jouissent donc, durant ce stade encore, d'une sorte de spontanéité. Cette spontanéité est intéressante au point de vue de la psychologie comparée, car, si les explications des stades précédents rappellent sans doute certains traits de la mentalité des peuples dits primitifs, le dynamisme de ce troisième stade rappelle d'une manière frappante certains aspects de la physique d'Aristote.

Le caractère principal de cette physique enfantine, et un caractère justement très péripatéticien, c'est que, pour l'enfant, tout mouvement suppose le concours de deux moteurs distincts, un moteur interne qui est l'activité propre du mobile, et un moteur externe, qui est la sollicitation physique dont le mobile est l'objet. Notons tout de suite que cette particularité s'explique comme un simple héritage des stades précédents. En effet, jusqu'ici également, tout mouvement s'expliquait par le concours de deux causes, l'une interne, l'autre externe par rapport au mobile. La cause externe n'était autre, jusqu'ici, que la volonté humaine, et la cause interne que le pouvoir d'obéissance et de réalisation, propre au mobile. Dorénavant la cause extérieure du mouvement cesse d'être morale et devient physique: ce n'est plus l'action des hommes, c'est un corps extérieur faisant pression sur le mobile, qui explique le mouvement de celui-ci. D'autre part, la cause interne du mouvement cesse aussi d'être conçue sur un type principalement moral: la soumission du mobile par rapport à la cause extérieure qui le met en mouvement n'est plus conçue comme une obéissance, mais comme une sorte de suggestibilité toute animale. Ainsi le vent fait avancer le nuage, parce qu'il excite à l'intérieur du nuage une tendance à avancer. Les corps ont ainsi un certain nombre de tendances spécifiques, et les causes extérieures qui agissent sur eux se bornent, pour ainsi dire, à déclencher ces tendances.

On voit que la théorie des deux moteurs, pour ainsi parler, semble dériver directement de ce mélange d'artificialisme et d'animisme, ou même de magie et d'animisme, propre aux stades précédents. D'autre part, cette conception amènera peu à peu l'enfant au mécanisme. Il faut donc nous attendre à ce que, grâce aux restes d'animisme dont nous venons de parler, il y ait tous les intermédiaires entre l'explication proprement animiste du mouvement, l'explication par les deux moteurs, et enfin l'explication mécaniste. On peut, en effet, concevoir les quatre termes de passage suivants, et nous allons voir qu'on les rencontre effectivement dans l'esprit de l'enfant:

1°. Les corps avancent tout seuls, pour accomplir leur fonction. Par exemple, les nuages "volent," comme les oiseaux.

2°. Ils produisent, pour avancer, un corps extérieur qui les pousse ou les entraîne. Par exemple, les nuages produisent du vent, et ce vent les pousse.

3°. Ils sont mus par un corps extérieur, mais, en se mouvant, ils renforcent l'action de ce corps extérieur. Par exemple, les nuages sont poussés par le vent, mais, en avançant, ils renforcent le vent.

4°. Ils sont mus par un corps extérieur, lequel suffit à expliquer leur mouvement. Par exemple, le vent pousse les nuages, et produit ainsi leur mouvement.

Comme on le voit, le premier de ces quatre types d'explication est celui des stades précédents, et le dernier est mécaniste. Les deux autres supposent le schéma du double moteur. Etudions donc ces types 2 et 3 qui caractérisent le présent stade et qui forment ainsi la transition entre l'animisme du dernier stade et le mécanisme du stade suivant.

Le type 2 paraît extraordinaire. On comprend mal, au premier abord, comment l'enfant en arrivera à concevoir que les corps, pour avancer, produisent d'autres corps qui les pousse ou les entraîne. En réalité, chose curieuse, ce type d'explication est extrêmement fréquent aux débuts du stade que nous étudions maintenant. L'existence de corps tels que ceux que conçoit l'enfant sous les noms de "vent," "chaleur," "courant," facilite d'ailleurs certainement la formation de ce schéma explicatif.

Il faut dire ici quelques mots des idées des enfants sur l'air et sur le vent. Vous vous rappelez l'expérience qui consiste à agiter un éventail devant l'enfant, dans une chambre aux fenêtres fermées, et à demander à l'enfant d'où vient l'air ainsi produit. Nous avons vu les réponses du premier stade. Ce qu'il faut préciser maintenant c'est que, jusqu'à 9 ans en moyenne, c.-à-d. jusque vers le milieu du stade que nous étudions en cet instant, ou bien l'enfant ignore qu'il y a de l'air dans la chambre et croit que l'air de l'éventail est créé par l'éventail lui-même, ou bien l'enfant déclare que même dans une chambre vide d'air l'éventail ferait du vent. Donc les objets en mouvement font de l'air. Or, chose curieuse, c'est précisément ainsi que l'enfant explique la formation du vent: le vent, c'est le résultat du mouvementdes nuages, des arbres, des vagues et même de la poussière! Les nuages, et parfois aussi les astres, en se déplaçant, font du vent. Les arbres se balancent tout seuls et font du vent. Les vagues et la poussière peuvent se soulever d'elles-mêmes et cela fait du vent.

Dans ces conditions, l'explication du mouvement devient relativement simple. En ce que concerne les nuages, les choses sont faciles à comprendre. Le nuage se met de lui-même en mouvement. Il produit ainsi un léger courant d'air, qui vient refluer derrière lui et qui le pousse en avant. Mu ainsi par l'action combinée de l'air et de sa force propre, le nuage va plus vite et provoque la formation d'un vent plus puissant, etc. Comme on le voit, l'enfant applique ainsi au nuage une expérience qu'il a faite sur lui-même: c'est que, lorsque nous courons, nous produisons un courant d'air. Mais l'enfant a l'impression d'être poussé par ce courant d'air et d'avoir créé lui-même cet air qui le pousse ainsi, alors que, comme on sait, le vide momentané qui déclenche ce reflux de l'air entrave au contraire la course.

En ce qui concerne les astres, l'enfant répète parfois cette explication, ou bien il suppose un courant chaud produit par le soleil et le faisant avancer. C'est ce qu'il veut dire en affirmant que "la chaleur" est cause du mouvement du soleil.

Les rivières se conforment à un schéma semblable. Elles avancent tout d'abord grâce à leur "courant," c.-à-d. toutes seules. Mais ensuite l'enfant réifie le courant, et en fait une sorte de substance qui pousse la rivière. Or, dans la plupart des cas et sans doute aidé en cela par le langage, il identifie ce "courant" à un "courant d'air" et en vient à croire que la rivière aussi est poussée par l'air qu'elle produit elle-même. Sans aller toujours jusque là, l'enfant affirme très souvent que les vagues font du vent, lequel soulève d'autres vagues, etc.; or, ces vagues favorisent la marche de la rivière.

Comme on le voit, de telles explications dérivent très directement de l'animisme du stade précédent, puisque, au point de départ, les choses sont censées se mettre d'elles-mêmes en mouvement. Mais il y a un gros progrès de fait, puisque, pour l'enfant, ce mouvement propre ne suffit pas: il faut un mécanisme matériel, sous la forme d'une cause motrice extérieure au mobile. Seulement, dans ces premières tentatives d'explication physique le moteur externe est conçu comme produit par le mobile lui-même. Nous sommes donc en présence d'un point essentiel de jonction entre le schéma animiste et le schéma dynamiste du double moteur.

Il suffira maintenant que l'air ne soit plus conçu comme créé par le mobile, pour que l'enfant atteigne le troisième des types d'explication distingués tout à l'heure: le mobile est mu par un corps extérieur, mais, en se déplaçant, il renforce l'action de ce corps extérieur. Autrement dit, par exemple, le nuage est poussé par le vent, conçu comme venant d'ailleurs. Mais, en avançant, le nuage augmente le courant du vent, ce qui a pour effet d'accroître l'élan du nuage lui-même.

En pratique et en ce qui concerne les mouvements de la nature elle-même, comme celui des nuages, celui des astres ou celui des rivières, nous aurons beaucoup de peine à distinguer ce type d'explication du précédent. Et cela va de soi. Aussi peut-il paraître absurde, parce que trop théorique, de faire cette distinction. Mais ce n'est nullement par goût de symétrie ou d'ordonnance logique que nous la faisons. En effet, en ce qui concerne certains mouvements artificiels, comme le mouvement d'une balle dans une chambre, ou d'une toupie, ou d'un corps retenu par une ficelle, l'enfant la fait de lui-même. Autrement dit, même lorsqu'il conçoit, vers 9, 10 et 11 ans, la chambre comme remplie d'air, et même lorsqu'il s'est rendu compte que les corps ne créent pas d'air en se mouvant, il continue à expliquer le mouvement par le reflux de l'air.

Or ce point a un certain intérêt historique, car, ainsi qu'on le voit d'emblée, cette explication ressemble d'une manière frappante à celle qu'Aristote donnait du mouvement des projectiles. On sait, en effet, que le mouvement des projectiles était fort embarrassant pour la physique d'Aristote. D'une part, en vertu de la théorie des deux moteurs, il fallait expliquer pourquoi la flèche, par exemple, continuait d'avancer une fois séparée de l'arc quoiqu'ayant quitté ainsi son moteur externe. En effet, l'arc ne peut agir sur la flèche à distance: or, selon la théorie du double moteur, le mobile ne peut avancer si son moteur externe ne l'accompagne pas ! D'autre part, pour Aristote, le lieu propre des corps graves étant le bas, la flèche devait tomber à terre sitôt sortie de l'arc. A cette double question, Aristote répond de la manière suivante: d'une part, le projecteur continue à agir, même à distance, sur le projectile, car il délègue son mouvement à l'air ambiant, autrement dit, il ébranle l'air et l'air pousse ainsi le projectile; d'autre part, le projectile lui-même déplace l'air en avançant, et l'air ainsi déplacé vient refluer derrière le mobile, qu'il projette donc plus avant. Ce dernier processus, ou processus du reflux de l'air, est appelé par Aristote l' *ἀντιπερίστασις*.

Or, on voit que ce processus est exactement celui que nous avons décrit jusqu'ici chez nos enfants, à cela près que le mobile ne crée pas d'air en avançant, mais produit simplement un courant dans l'air ambiant. Seulement, comme nous venons de le voir, l'explication par le reflux de l'air subsiste chez l'enfant même après qu'il a compris ce dernier point. Nous avons tenu, pour nous en assurer, à poser à l'enfant le problème même d'Aristote: pourquoi, lorsqu'on lance une balle au loin, ne tombe-t-elle pas à terre en quittant la main qui l'a jetée? La plupart des enfants de 9 et 10 ans m'ont dit que la balle continuait sa route à cause du courant d'air. Les uns attribuaient le courant d'air à la main qui lance, les autres (et les plus nombreux) à la balle elle-même. Et cependant ces enfants affirmaient que la balle ne "faisait" pas d'air. Selon eux, dans un local vide d'air, la balle serait tombée à terre sitôt lancée ! C'est donc exactement l'explication d'Aristote, que nous pouvons ainsi considérer non comme une explication savante, mais comme une simple codification des idées du sens commun de son temps.

Venons-en maintenant, pour terminer l'étude de ce stade, à certaines explications dans lesquelles éclate un dynamisme particulier à l'enfant, là où nous aurions attendu un pur mécanisme.

Disons d'abord que, lorsque l'enfant attribue simplement au vent le mouvement des nuages, des astres et des rivières, sans plus admettre que ce vent est dû aux mobiles eux-mêmes, l'explication n'est pas d'emblée mécaniste. Souvent l'enfant accepte encore l'idée que, sans aucun vent, ces corps pourraient se mouvoir faiblement. On a donc souvent l'impression que, pour l'enfant, le vent déclenche la force intérieure de ces mobiles plutôt qu'il ne leur communique cette force motrice du dehors. Mais rapidement l'enfant renonce à ces réserves, et nous passons alors au quatrième stade que nous aborderons à l'instant.

Demandons ensuite à l'enfant comment il se fait que les nuages restent suspendus en l'air, ou que les bateaux flottent. Chose curieuse, entre 8 et 10 ans, c.-à-d. justement à l'âge des explications par le double moteur, que nous venons d'étudier, l'enfant invoque beaucoup plus des causes dynamistes que des raisons mécanistes. La suspension ou la flottaison ne sont pas encore considérées, en effet, comme dues au poids relatif du corps envisagé par rapport à la densité du milieu dans lequel il est plongé. Le poids intervient bien un peu, mais comme symbole de force. La vraie explication, pour l'enfant, doit faire intervenir une sorte de vol plané. Les nuages restent en l'air parce qu'ils vont assez vite pour être soutenus par leur propre mouvement, ou par le courant qui les entraîne. Les bateaux, de même, flottent parce qu'ils vont vite. On objecte alors naturellement à l'enfant qu'il y a des nuages et des bateaux immobiles. "C'est vrai, répond le raisonneur, mais ça ne va qu'un moment. C'est l'élan. S'ils s'arrêtent trop longtemps, ils tombent." Cela n'arrive d'ailleurs jamais, mais on se rappelle que pour l'enfant tout est pour le mieux dans l'harmonie des choses.

Demandons enfin à l'enfant pourquoi l'eau d'un verre d'eau change de niveau et s'élève quand on immerge un gros caillou. Jusqu'à 9 ans, l'enfant ne pense pas à cette raison si simple que le caillou occupe un certain espace et déplace l'eau d'autant. La réponse est uniquement: "L'eau monte parce que le caillou est lourd." En apparence cela revient au même, et il semble que l'enfant veuille simplement dire que le caillou, étant lourd, coule au fond et occupe ainsi la place de l'eau. Mais en fait, l'idée de l'enfant est de nouveau plus dynamique que statique. Il pense que le caillou pèse sur l'eau et crée un courant qui fait monter l'eau. D'après l'enfant, un petit sac de grenaille plus lourd, quoique plus petit, qu'un morceau de terre glaise fera plus monter l'eau que la terre glaise; et cependant tous deux vont au fond. Ou encore, un caillou retenu à mi-hauteur de la colonne d'eau ne fera pas monter l'eau parce qu'il est suspendu par un fil et ne pèse pas sur l'eau.

Bref, on s'aperçoit que, dans tous les domaines, les réponses de ce stade témoignent d'un dynamisme intégral, tel que les corps soient considérés comme doués de forces, d'activités, de spontanéité. Il y a un gros progrès de fait, par rapport au stade précédent, puisque l'explication devient physique. Mais l'explication physique qui apparaît ainsi pour la première fois dans sa généralité reste encore imprégnée d'animisme.

IV. LE STADE DE L'EXPLICATION MÉCANISTE

Durant le quatrième stade, par contre, l'explication physique commence à s'épurer. Il est assurément difficile de situer avec précision les débuts de ce stade. Certains faits l'annoncent dès 7–8 ans. La causalité mécanique remplace le dynamisme dans certains domaines dès 9–10 ans. Nous pouvons cependant donner l'âge moyen de 10–11 ans comme l'âge de l'élimination du dynamisme animiste, autrement dit comme le point de départ effectif du quatrième stade.

Nous serons brefs sur ce stade, car c'est celui au cours duquel l'enfant commence à penser comme l'adulte. Il est néanmoins utile de noter quelques convergences entre les dates auxquelles apparaissent les principaux caractères de la causalité mécanique.

Tout d'abord, c'est vers 9–10 ans que l'explication des mouvements naturels devient mécaniste. Vers 10 ans, l'enfant se refuse à expliquer comme précédemment la formation du vent par le mouvement des nuages ou des arbres: vers 9–10 ans, l'enfant explique le mouvement des nuages en disant simplement que c'est le vent qui les pousse. Il n'y a plus trace de spontanéité prêtée au nuage ni du schéma péripatéticien du reflux de l'air. Au même âge environ, l'enfant attribue au vent également le mouvement des astres, lorsque l'école ne lui a pas imposé l'explication copernicienne, laquelle reste, à cet âge, toute verbale. Dès 10–11 ans, l'enfant comprend que le mouvement des rivières est dû au poids de l'eau, qui entraîne la rivière vers le bas, et non à une "force" ou à un "courant" mystérieux. C'est également vers 10–11 ans que l'enfant explique par la densité respective des corps la suspension des nuages et la flottaison des bateaux.

Or, si nos hypothèses de travail sont exactes, ces conquêtes du mécanisme, vers 10 et 11 ans, ne doivent pas être dues simplement à une élimination du subjectivisme et à un progrès de l'empirisme, mais bien à un progrès de la raison qui amène l'enfant à se débarrasser à la fois des adhérences subjectives de sa pensée et de l'apparence empirique des choses. Nous avons vu, en effet, que la causalité débute, chez l'enfant, par le maximum de subjectivisme uni au maximum de phénoménisme. Nous avons vu, d'autre part, en examinant successivement nos quatre stades, que les adhérences subjectives de la causalité disparaissent peu à peu, de l'efficace à l'animisme et à l'artificialisme, de l'animisme au dynamisme et du dynamisme au mécanisme. Il convient maintenant de voir si l'empirisme disparaît d'autant.

A cet égard, le passage du stade de l'explication morale au stade de la causalité dynamiste comporte déjà quelques enseignements. C'est au cours de ce passage que l'enfant renonce à croire à l'apparent mouvement des astres, qui paraissent nous suivre, pour construire, grâce à sa raison seule, l'hypothèse d'un mouvement astronomique indépendant de nous. Les explications du mouvement par le reflux de l'air ou de la suspension par le vol plané sont également des constructions rationnelles succédant à l'empirisme naïf du stade précédent. Mais il est clair que ces constructions sont encore singulièrement proches de l'apparence empirique. C'est donc le passage du dynamisme au mécanisme qui sera le plus instructif au point de vue qui nous occupe maintenant.

Or deux faits sont particulièrement suggestifs à cet égard. Il s'agit de deux transformations essentielles dans le raisonnement scientifique de l'enfant, et des deux transformations qui sont précisément contemporaines de l'avènement du mécanisme dans la pensée enfantine.

Le premier de ces faits est l'apparition des principes de conservation. Comme nous l'avons entrevu, l'idée de poids joue un grand rôle dans la physique de l'enfant, mais dans un sens très spécial: le poids est le symbole de la force et de la force quasi vivante. Mais, d'autre part, le poids sans la mesure du poids est quelque chose qui dépend singulièrement des apparences, et l'enfant est justement très peu exigeant et très imprécis dans ses estimations du poids de corps. Un gros objet creux lui paraît d'autant plus léger qu'il l'a cru plus lourd, etc. On sait que Demoor a fondé sur cette observation un test intéressant d'illusion de poids, et que de Sanctis a montré que, jusque vers 8 ans, le poids paraît à l'enfant proportionnel au volume. Enfin, il est clair que, jusque très tard, et sans doute jusqu'aux premières expériences de physique et de chimie, l'enfant n'aura aucune notion de la mesure exacte du poids. Demandons-nous donc si, spontanément, il se conduit vis-à-vis du poids en pur empiriste, c.-à-d. en s'en tenant aux apparences seules, ou en rationaliste, c.-à-d. en posant des principes de conservation, invérifiables pour l'expérience telle qu'il la pratique, mais simplifiant l'interprétation de cette expérience.

Pour ce faire, prenons deux boulettes de terre glaise ou de cette pâte préparée pour les jeux d'enfants sous le nom plasticine, et rendons ces boulettes identiques en poids et en volume. Nous faisons constater cette identité à l'enfant, qui soupèse les boulettes et déclare qu'effectivement elles ont le même poids. Ensuite, nous laissons à l'une des boulettes sa forme, mais nous roulons l'autre entre nos mains jusqu'à ce qu'elle ait pris l'aspect d'un long boudin. Nous demandons alors à l'enfant si les deux boulettes ont encore le même poids, et pourquoi oui ou non.

Les résultats de cette expérience sont très curieux, parce qu'ils varient beaucoup suivant nos stades, et que, à chaque stade, l'enfant est très sûr de lui, au point qu'il considère comme des absurdités les réponses des stades précédents. Or l'évolution des réponses procède très nettement de l'empirisme au rationalisme.

Jusque vers 7–8 ans, en effet, l'enfant se soumet servilement à l'apparence sensible. Le boudin lui paraît plus léger que la boulette, parce qu'il est plus mince et plus effilé. Dès lors l'enfant affirme que les deux objets n'ont plus le même poids et que le boudin est le moins lourd. On rappelle alors au sujet que, précédemment, les boulettes avaient le même poids. Il s'en souvient d'ailleurs bien, mais ce fait ne lui paraît impliquer en rien que les objets aient conservé ce poids. L'enfant explique que le boudin est devenu "plus mince" et que, par conséquent, il est "plus léger." Bien plus, certains sujets affirment que le boudin est devenu "plus petit." En cherchant à comprendre cela, nous nous sommes aperçus que la matière même ne se conserve pas pour l'enfant de cet âge. Nous demandons, en effet, à l'enfant, en désignant le boudin et la boulette, si ces deux objets, qui avaient auparavant exactement le même volume, ont conservé la même quantité de terre glaise ou de plasticine. L'enfant de ce stade affirme que non: la boulette, pour eux, est "plus grosse" que le boudin, c.-à-d. qu'il y a plus de terre, etc., que dans le boudin. On demande alors à l'enfant où a bien pu passer la terre qui a ainsi disparu: il vous regarde, interloqué par cette question bizarre, mais ne modifie en rien sa réponse.

Durant le stade dynamiste, c.-à-d., en fait de 7 à 10 ans, les choses changent un peu. L'enfant affirme la conservation de la matière. Il affirme donc qu'il y a autant de terre dans le boudin que dans la boulette, et, lorsqu'on lui demande pourquoi, il ajoute en riant: "Mais il y en avait la même chose avant!" Le principe de conservation lui paraît donc évident, et il rit de ce qu'on puisse en douter. Un an auparavant, peut-être, il aurait cependant dit le contraire. Bien plus, cette conservation de la matière ne lui paraît pas entraîner celle du poids, et il continue, jusque vers 10 ans en moyenne, d'affirmer que le boudin est moins lourd que la boulette!

Vers 10 ans, au contraire, l'enfant affirme que les deux poids sont restés égaux, et il sourit de la question, tant il lui paraît évident que le poids se conserve.

Ces faits sont donc intéressants. De tels principes de conservation marquent assurément un grand progrès dans l'esprit de l'enfant, quoique, d'après la physique, la masse seule se conserve, et non le poids, et que la masse même, comme on sait depuis Einstein, varie avec la vitesse...Le progrès qu'accomplit l'enfant est assurément en relation avec le développement de l'explication mécaniste, puisque c'est le même effort de rationalité qui pousse l'esprit à nier, malgré les apparences, la vie spontanée des corps et leurs changements qualitatifs, au profit d'un univers déterminé et quantitatif. Or, comment l'enfant est-il parvenu à ces principes de conservation? Par l'expérience? Assurément pas: il faudrait des balances très fines et un souci extraordinairement précoce de la précision, pour que l'enfant soit arrivé à cette loi, comme il arrive à une loi physique telle que: la glace fond avec la chaleur, etc. Il s'agit donc bien d'un pur décret de l'intelligence, et non d'une découverte empirique. La seule question est de savoir pourquoi ce décret apparaît si tard? Mais justement, la psychologie de l'enfant nous apprend que la raison ne se construit que peu à peu, au contact des faits, mais non sous leur seule pression.

Le second des phénomènes dont nous voulions parler est l'apparition des notions relatives, c.-à-d. des jugements de relation physique, par opposition aux jugements prédicatifs absolus. Nous pouvons prendre à nouveau, comme exemple, la notion de poids.

Lorsque l'on demande à l'enfant pourquoi les bateaux flottent, l'explication fait très vite intervenir le poids. Mais, comme on a vu déjà, l'enfant commence (avant 7–8 ans) par affirmer tout à la fois que les petits bateaux flottent parce que légers et les grands parce que lourds. C'est que l'enfant ne songe nullement encore à comparer le poids des bateaux au poids de l'eau elle-même. Pour lui, la lourdeur ou la légèreté sont des qualités absolues, que l'on a ou que l'on a pas, et qui entraînent leurs effets par le fait même de leur présence: ainsi les grands bateaux, étant lourds, ont la force de se soutenir d'eux-mêmes, tandis que les petits, n'étant pas lourds, sont portés par l'eau, qui est forte. Durant le stade dynamiste, et dès 7–8 ans, l'enfant commence, par contre, à faire des comparaisons. Les bateaux flottent tous parce qu'ils sont légers, même les grands, parce qu'un grand bateau est léger par rapport à l'ensemble de l'eau d'un lac ou de la mer. Mais sur un étang, un bateau à vapeur coulerait. Cette explication est plus ou moins mélangée aux explications par le vol plané, dont nous avons déjà parlé. Elle marque assurément un progrès dans le sens de la relativité, mais, comme on voit, elle reste assez rudimentaire, puis qu'elle ne rend pas compte du fait qu'un caillou coule tandis qu'un bateau flotte. L'enfant s'en rend bien compte, et c'est pourquoi il fait intervenir le vol plané comme cause adjuvante de la flottaison.

Or, vers 10 et 11 ans, c.-à-d., justement à l'époque où se précise le principe de conservation du poids, l'enfant commence à concevoir le poids comme relatif à un volume donné. Il dira, par exemple, qu'un bateau à rame est plus léger qu'un caillou, parce qu'il est vide, et que le bois est plus léger, à volume égal, que la pierre. Les choses sont en fait assez compliquées, parce que, avant d'en venir là, l'enfant commence par admettre qu'un corps diminue de poids, absolument et pas seulement relativement, lorsque sa forme s'étire (la quantité de matière restant la même). Mais le résultat de ces réflexions de l'enfant est que, vers 10 et 11 ans, la notion de densité est acquise, c.-à-d., que le poids du flottant est toujours estimé en fonction du volume d'eau correspondant.

Bref, la notion de poids commence par être absolue, à la manière du "haut" et du "bas" absolus d'Aristote, puis elle se mue en une pure relation. Or il va de soi que cette logique de la relation est seule adéquate à la réalité physique, par opposition au réalisme verbal et conceptuel qui caractérise les débuts de cette évolution. Mais il est évident, d'autre part, que cette élaboration des relations n'est pas due à de simples constatations empiriques, puisque les raisonnements qu'elle suppose aboutissent à la construction d'un monde quantitatif et rationnel qui vient remplacer l'univers de l'apparence sensible. Il est d'autant plus intéressant de noter que cette transformation s'opère au moment où se constitue le principe de la conservation du poids et au moment où l'explication mécaniste supplante le dynamisme anthropomorphique des stades précédents.

CONCLUSION

Il semble ainsi que nous ayons vérifié, au point de vue purement psychologique auquel nous nous sommes placé, les hypothèses que nous faisions en étudiant les débuts de la causalité chez l'enfant.

Au point de départ, le moi est entièrement confondu avec les choses. Les relations qui s'établissent entre l'organisme et son milieu donnent ainsi naissance à une forme primitive de causalité telle que les liaisons purement phénoménistes et empiriques s'accompagnent nécessairement du sentiment de l'efficace des efforts et des gestes personnels. Dans la suite, l'enfant se libère à la fois de ces adhérences subjectives et des apparences sensibles. La causalité procède ainsi, d'une part, de la magie à l'animisme et à l'artificialisme, de l'animisme au dynamisme et du dynamisme au mécanisme, ce qui constitue bien une élimination du moi au profit de l'objectivité. Mais, en même temps, à l'empirisme grossier des débuts succède la construction d'un monde de relations tel que l'enfant substitue à l'expérience immédiate des explications faisant intervenir des principes rationnels de conservation ainsi qu'une sorte de déterminisme implicite fondé sur la comparaison quantitative. A cet égard, la causalité est bien plus le produit d'une construction intellectuelle que la donnée immédiate d'une expérience interne ou d'une expérience externe.

On ne peut pas ne pas être frappé du parallélisme qui existe dans les grandes lignes entre ce développement de la causalité chez l'enfant, et le développement de la notion de cause dans l'histoire des sociétés humaines. C'est ce que nous voudrions souligner, pour conclure cette rapide esquisse. Mais ici, la plus grande prudence s'impose, et cela pour deux raisons.

En premier lieu, si vraiment il y a quelque rapport entre la causalité chez le petit enfant et la causalité dans la mentalité primitive, gardons-nous de croire, avec Stanley Hall, que cette mentalité ait pu se transmettre à l'enfant civilisé contemporain, par voie d'hérédité. Cette hypothèse est, au point de vue biologique, certainement très fragile.

En second lieu, gardons-nous d'admettre que, si l'enfant parvient à certains principes rationnels, il y arrive entièrement de lui-même. Ces principes ne lui ont certes pas été dictés. Mais, dans l'ambiance rationnelle dans laquelle vit l'enfant civilisé, il va de soi que l'arrivée de l'enfant au dernier des stades que nous avons décrits est singulièrement facilitée par la pression continue de la mentalité adulte.

Cela étant, il semble que nous puissions néanmoins avancer certaines choses. Comme on le sait, Durkheim et les sociologues de son école ont admis que la causalité devait à la société sa genèse et son développement. C'est le sentiment de la contrainte sociale qui aurait suggéré aux primitifs la croyance en des forces occultes: les croyances magiques et animistes en général. Ce serait le progrès des sociétés qui expliquerait ensuite l'élimination de ces formes anthropomorphiques ou sociomorphiques de la causalité, et l'avènement du mécanisme rationaliste. Or, pour notre part, la comparaison entre le développement mental de l'enfant et le développement historique de la causalité, nous paraît parler beaucoup plus en faveur de la seconde de ces thèses qu'en faveur de la première.

En ce qui concerne la genèse de la causalité, il nous semble, en effet, bien difficile d'attribuer à la société la formation de cette notion fondamentale de l'esprit, puisque le bébé construit déjà des relations magico-phénoménistes au moment où il commence seulement à imiter autrui. Il nous paraît bien délicat de nier toute analogie entre la magie sociale des primitifs et les croyances de l'enfant en l'efficace personnelle, ou entre les participations primitives et les participations enfantines. Il y a, quoi qu'en ait dit Durkheim, un animisme enfantin. L'artificialisme enfantin suppose assurément la société, puis qu'il est né des représentations que les petits se font de la puissance adulte, mais il suppose aussi la mentalité propre à l'enfant lui-même. Bref, il est bien difficile de ne pas voir dans la mentalité primitive des restes de la mentalité enfantine. Comme l'a si bien dit M. F. C. Bartlett, le tort de l'école sociologique de Durkheim n'est pas d'avoir mal analysé le primitif, c'est de n'avoir pas scruté suffisamment l'esprit du civilisé, et surtout, ajouterons-nous, l'esprit de l'enfant. En ce qui concerne en particulier la causalité, les sociétés primitives semblent beaucoup plus avoir retravaillé, consolidé et stylisé les différentes formes de la causalité enfantine que d'avoir créé de l'absolument nouveau.

Mais si la causalité primitive est infantile dans sa source, il nous paraît indéniable que la causalité rationnelle doit son essor aux influences sociales. Il ne suffit pas, en effet, de dire avec les philosophes que la causalité est une forme de l'esprit. Il faut encore montrer sous l'influence de quelles conditions cette forme a pu prendre conscience d'elle-même. Or, à cet égard, la psychologie de l'enfant nous donne un enseignement utile: c'est en se débarrassant de son moi que l'enfant arrive à penser les choses. D'autre part, les sociologues nous apprennent ceci: c'est en se débarrassant de la contrainte sociale et du conformisme obligatoire que les sociétés sont arrivées à la raison. Les deux thèses si différentes sont moins contradictoires qu'il ne semble, car, chez l'enfant, l'égocentrisme de la pensée et la soumission à l'autorité adulte vont curieusement de pair. C'est donc la libre discussion qui crée la raison: la réflexion logique n'est qu'une discussion intériorisée. En effet, la libre discussion élimine à la fois l'autorité extérieure et l'égocentrisme intellectuel. C'est en ce sens que, dans la mesure où la causalité scientifique est un produit de la raison, elle est aussi un produit de la société.

A CONATIVE CRITERION FOR THE DISCRIMINATION OF THE INSTINCTS

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INTRODUCTION.

It is generally agreed among psychologists that human conduct is largely influenced, even determined, by certain innate tendencies to action which arise, each in its own peculiar set of appropriate circumstances, and impel, each to its own peculiar type of purposive behaviour. These tendencies we call instincts. But there the agreement ends. Not only is there difference of opinion on the fundamental theoretical problems of the nature and ground of the instincts, but on the much simpler and very practical problems of how many of these innate impulses we have, and to what activities they lead, there is also profound disagreement. Yet this is just the information which is demanded of the psychologist. If it be true that man has a number of native impulses to certain types of behaviour, it is of obvious importance that we should know what those impulses are. If psychology is ever to offer a secure basis for educational and sociological theory we must be able to give an adequate and definite answer to the question[1].

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[1] The present confusion on this subject of the criteria of an instinct may be judged by a comparison of those of McDougall, mentioned in this paper, with those of Shand, *Foundations of Character*, p. 219, and Rivers, *Instinct and the Unconscious*, p. 48.

Probably the greatest contribution to this problem made in our time has been that of McDougall in his *Social Psychology*. His definition of instinct there given is, in substance, now widely adopted, and the list of instincts which, on the basis of this definition, he has drawn up is a very promising approach to the solution of the problem. But his list is still very far from receiving universal assent even in its main outlines[1], and this is due to the lack of any really satisfactory *psychological* criterion of an instinct by which its validity may be demonstrated or criticized.

I. THE CRITERION PROPOSED.

1. *The unsatisfactory nature of the 'affective' criterion.*

McDougall's definition[2] points out three distinctive features of instinctive behaviour. (1) The attention of the creature is attracted and held by objects of a certain class; *i.e.* the instinct is excited by a specific type of stimulus. (2) The action of this stimulus tends to rouse an emotional excitement of a peculiar quality. (3) The creature tends to manifest a specific mode of behaviour in relation to the object attended to. It is his insistence on the second feature that is characteristic of McDougall's account of the instincts. When the first and the third—stimulus and reaction—are taken alone the psychologist is inevitably led to enumerate a bewildering multitude of instincts[3].

McDougall's emphasis on the emotional aspect of the instinct-process does much to reduce this chaos to order. It is an example of the importance in Psychology of the peculiarly psychological standpoint—that of the individual experient and the introspective observation of his own experiences.

But valuable as the affective criterion is it cannot be claimed as decisive. It is much more useful on the positive side than on the negative. Fear, anger, tender emotion, etc., are such distinct mental states that, when we have learned to attribute them to the working of instincts, they very clearly brand the instinct to which they belong; though even here there is the objection that, frequently, action which is indubitably instinctive takes place without any emotion being felt at all[4]. A more serious difficulty, however, is that many important types of instinctive activity are accompanied only by an emotional excitement which, even when strong, possesses no clear distinctive tone. This fact led, in his *Social Psychology*, to what looks like a peculiar distortion of emphasis in McDougall's treatment of the instincts. The seven which have a clearly defined emotional colouring—Flight, Repulsion, Curiosity, Pugnacity, Self-Assertion, Subjection and Parental Instinct—are given a prominence which some of them do not deserve; while Sex, Food-seeking, Gregariousness, Acquisition and Construction are thrust into the background. Indeed, if the principle be strictly adhered to that "each of the principal instincts conditions some one kind of emotional excitement the quality of which is specific or peculiar to it[1]," it is difficult to see what right these last-named five have to appear in the list at all. It seems that all we can say with certainty is that instinctive action tends to be accompanied by emotional excitement and that in certain types of instinctive behaviour this excitement is always of a specific quality. This suggests, though it does not prove, that the peculiar kinds of emotion belong each to a particular instinct. But it leaves us without any guidance at all regarding those instincts not thus specially marked.

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[1] Thorndike, *Educational Psychology*; Rivers, *Instinct and the Unconscious*; F. MacCurdy, *Problems in Dynamic Psychology*.

[2] *An Introduction to Social Psychology*, ch. II; *Outline of Psychology*, ch. IV.

[3] J B. Watson, *Behaviourism*; Thorndike, *Educational Psychology*.

[4] Shand, *The Foundations of Character*; Drever, *Instinct in Man*.

In distinguishing among human emotions those which are primary (associated with simple instinctive impulses) McDougall suggests two further criteria as of great assistance. (1) The appearance of a similar emotion or impulse in the higher animals. (2) The occasional appearance of the emotion or impulse in exaggerated form, apart from such general hyperexcitability as is displayed in mania. But these cannot be taken as independent criteria of an instinct. The second of them, even if its theoretical basis be admitted as valid, can only be of help so far as instinctive actions are marked by emotions of clearly distinguishable quality; it is subject, therefore, to all the limitations of the affective criterion. The former is open to the objection that we only estimate the animal's emotional experience from its behaviour, *i.e.* from the way in which it responds to a stimulus. So this criterion merely carries us back to the first and third features of an instinct-process enumerated in McDougall's definition, and emphasizes the importance of the comparative study of these. These secondary principles, then, in no way make up for the intrinsic shortcomings of the affective criterion.

2. *The 'end' as the distinguishing mark of an instinct.*

In his discussion of the instincts in his more recent *Outline of Psychology* McDougall lays less emphasis on the affective criterion than in his earlier work. He stresses instead the first and third features of an instinct process, the facts that each instinct has its own peculiar kind of stimulus and its own peculiar type of purposive reaction. "We must define any instinct," he writes, "by the nature of the goal, the type of situation that it seeks or tends to bring about,-as well as by the type of situation or object that brings it into activity[1]."

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[1] McDougall, *An Introduction to Social Psychology.*

But, he goes on to ask, "What degree of generality may be assigned to the goal of an instinct?" Faced with this problem some writers have decided to group instinctive tendencies under the biological ends they serve, the commonest classification of this kind being into Self-preservative tendencies, Race-preservative tendencies and the Herd tendencies[2]. MacCurdy defends this grouping on the grounds of its simplicity and convenience[3]. But this is to surrender the attempt at a *psychological explanation* of instinctive behaviour. Merely to describe conduct in terms of stimulus and reaction and then to classify these reactions according to their biological utility is not Psychology. Psychology must not only study behaviour; it must study behaviour from the standpoint of the individual experient. It must not only describe objectively, but subjectively also. If it is to *explain* anything that explanation must be in terms of lived experience. Instinctive actions are biologically purposive and it is therefore perfectly legitimate to classify them according to the ends they serve; but if the classification is to be useful to Psychology it must be made in terms of ends which enter into the individual experience. This, at the merely instinctive level, self-preservation and race-preservation never do.

This psychological criterion McDougall still seems to seek in the emotion accompanying the instinctive activity. There is, however, greater value than he has explicitly observed in his suggestion that an instinct is to be defined "by the nature of the goal, the type of situation, that it seeks or tends to bring about." Attention to this purposive aspect of the instinct-process will reveal an end which enters into the experience of the individual in every complete instinctive action and which serves adequately and definitely to define that action. This is the end-result which, being reached, causes the conation to cease; *i.e.* the *end-experience* in which the conative process finds its completion and in which the creature finds satisfaction. This is not necessarily the end which the biologist supposes the activity to subserve; nor is it the fragmentary end which, from time to time, the creature envisages and pursues. *It is the end-experience in which the conation is finally satisfied.*

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[1] *Outline of Psychology*, p. 119.

[2] J. T. MacCurdy, *Problems of Dynamic Psychology*; Rivers, *Instinct and the Unconscious*; Tansley, *The New Psychology*.

[3] J. T. MacCurdy, *op. cit.*

This end, or final satisfying experience, I propose should be taken as the fundamental criterion of an instinct. In doing this I believe I am merely making explicit, and pointing out as fundamental, a principle which McDougall has used without clearly formulating. He takes the objectively observed goal of the instinct as a criterion, and argues that the point at which it has been reached is the point at which the conation ceases[1]. But he does not single out this point as marking that *experience* which, for the individual, contains the true meaning of the whole striving process; and he does not insist that it is the nature of this experience that should be taken as primarily differentiating one instinct-process from another. So far as instinct-processes differ in experience he still seeks that difference in the quality of the accompanying emotion. This difference, as we have pointed out, is not always to be found. But the conation, as well as the emotion, forms part of the complete experience of being driven by an instinctive impulse. Conations, however, can only be distinguished from each other by the ends toward which they impel; and these ends often do not enter consciousness until they are realized. The youth stirred by sex, for example, often does not know what he wants and may try to satisfy himself by hunting, wandering or the society of male friends. But though he does not know the nature of the conation activating him, it yet has a distinct nature of its own, for it is such that only the achievement of one peculiar end can adequately satisfy it. The end is, as it were, latent in the conation from the beginning. But, whether latent or explicitly understood, full satisfaction of the instinctive impulse is impossible until that end is realized in the appropriate end-experience. This end-experience is thus the end or terminal of the process of instinctive activity and also the end or goal of the instinctive striving. In it and it alone is defined the meaning of the individual's instinctive behaviour and of the psychological structure from which that behaviour springs.

The "end-experience in which the conation is finally satisfied" is an end which may not enter the creature's consciousness during the whole process of instinctive activity until it is at length attained, or just about to be attained. But when at last it is reached there is a pleasurable sense of satisfaction and a perhaps sudden cessation of the conation. The activity comes to an end. Other ends there will be, scattered along the line of development of the activity, some—or all—of which are more or less clearly envisaged as such. There is a pleasurable satisfaction in achieving them, *but they bring no condition of final rest*; rather the achievement of these particular ends forms a stimulus to further continued activity, to pursuit of some further end, until at last the final goal is reached. This goal it is which, I propose, should be taken as the criterion of an instinct.

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[1] *Outline of Psychology*, ch. v.

In human instinctive processes this goal is usually fairly clearly foreseen from the beginning. When a man is stirred by anger, curiosity or disgust he usually knows what he wants and takes means to secure it. The end experience in which his conation can be terminated, in which he can find satisfaction, is an end for him in the sense of being a goal consciously sought. In the lower animals it is only an end or goal in the accommodated sense of being the result which the instinctive efforts of the creature are tending towards and with accomplishment of which they will cease. This does not mean that instinctive striving is entirely random or blind. It seems to be far from that. To watch a bird building a nest is instructive on this point. It hops about restlessly until its attention is attracted by a certain piece of stick or grass, which it then hops towards and picks up. After this there is usually a little hesitation; the bird turns its head this way and that and may even hop about for a few minutes with the piece in its beak. Then it flies away to a tree or bush and, after a good deal of uncertainty, finally deposits its burden. So the process goes on. Now, the bird can have no clear idea of why it does all this. But it is difficult to escape the conviction that as it hops towards the twig which has riveted its attention it does so *with the conscious purpose* of picking it up, that as it flies away with the twig it does so with the purpose of reaching a bush or tree, that as it finally moves toward the spot where it deposits its burden it does so with the purpose of setting it down there. In this laying of the first twig, therefore, there would seem to be a succession of ends envisaged and attained, the attaining of one being followed by a pleasurable satisfaction, a period of uncertainty and of tentative movements, and then the envisaging of the next end. Yet these ends are but partial and the satisfactions temporary. The total conative process must go on until the final goal of the instinct is reached or the creature is left painfully dissatisfied. This final goal, from the standpoint of the individual experient, is the experience with which the conation ends. It may legitimately be called the goal or end of this instinct; and it is according to these ends that it is proposed that instinctive behaviour should be classified. For every distinct type of instinctive behaviour (*i.e.* for every different instinct) there is a certain type of end-experience in which alone satisfaction can be found. I propose *to distinguish the instincts from each other according to the different types of end-experience in which alone their respective conations can be satisfied.*

This criterion, it must be remembered, is not intended to distinguish native instinctive reactions from other kinds of activity, but to distinguish one kind of native tendency to action from another, and thus to discover what kinds of native tendency to action man (or any other animal) has, in what circumstances these impulses arise and to what kinds of behaviour they lead. Further, when this work has been satisfactorily accomplished, the same criterion will enable us to examine more sophisticated human conduct with a view to ascertaining what instinctive impulses, if any, motivate it.

3. *Use and advantages of the proposed 'conative' criterion.*

The advantages of the criterion are numerous. In the first place it is definitely psychological, for the end referred to is one that enters into the *experience* of every complete instinct-process. This end is also one which, in different degrees according to the intelligence and knowledge of the creature, is foreseen and deliberately sought; the criterion is therefore one which can be applied by the essentially psychological method of introspection as well as by objective observation. Above all, the criterion enables us to distinguish instincts by reference to their fundamental feature as processes of striving toward definite, though not necessarily clearly foreseen, ends. The instinctive activity is a psychological whole the meaning of which is found only in its end. The accompanying motion, the motor mechanisms employed, and the biological utility served are accidental or extraneous features of this process; the end is intrinsic and until it is experienced the process is not complete. To take the experienced end as criterion and basis of classification is therefore logically the most valid proceeding as well as being empirically the most serviceable.

The application of this criterion will, I believe, enable the psychology of instinct to be placed on a much sounder footing. It seems to me to make possible the decision of many points of disagreement among psychologists and to lead to the making of some finer distinctions among instincts than have been generally accepted hitherto. It would be impossible, within the limits of this paper, adequately to discuss all the more important instinctive tendencies of man and to classify them, by means of this criterion, according to the instinct to which they belong.

Such an investigation will however, I believe, reveal the *principal*[1] instincts of man to be the following: Nutrition, Hunting[2], Wandering[3], Acquisition, Escape, Repulsion, Curiosity, Constructiveness, Pugnacity, Sex, Parental, Appeal, Herd, Display, Domination and Subjection. This, it will be observed, adds Hunting and Wandering to McDougall's list in the *Outlines of Psychology* and divides his instinct of Self-assertion *or* Self-display into the two separate instincts of Domination *and* Display. I shall confine my further remarks to these points where my list differs from McDougall's and to a brief discussion of Herd[4] instinct, some important problems connected with which I believe the proposed criterion enables us to solve. This discussion, besides being on matters important in themselves, will serve to further clarify the nature of the suggested criterion by giving some examples of its application[5].

II. Illustrative discussion of certain instincts in the light of the Criterion Proposed.

1: *The Wandering Instinct.*

Though the needs of nutrition are probably the primary source of the Wandering impulse and are still very common stimuli of it, it is noteworthy in very many animals that wandering has become an end in itself. Well-fed animals will wander *away* from their accustomed sources of food. *The end in which satisfaction is found seems to be merely the finding of a change in environment or habitat.* In certain birds this instinct takes at specific seasons the special form of the migratory tendency. It has been established that even the young birds which have never known the warmer climes will, quite independently of the example of their elders, take their flight in the autumn. In the spring the impulse returns in its complementary form and they return. Similar migratory tendencies are also manifested by certain animals, especially those living in the colder parts of the temperate zone. Herbivorous animals usually wander considerable distances while feeding—certainly much more than is necessary to secure an adequate supply of food—and whole herds traverse considerable distances in wandering from place to place. The activity is one developed, undoubtedly, primarily in the service of the animal's nutrition (and to a lesser extent the needs of sex); but in the higher animals and in man it certainly seems to be pursued for the sake of its own end—that of change in surroundings. In us and in them the impulse arises, at times, to seek some change in environment and a satisfaction may be found in obtaining that change, even though it is but temporary and contributes nothing to the increase of food supply. The original connection of the instinct with that of nutrition, however, is perhaps to be seen in the fact that we humans find the satisfaction of an outing always made more complete if it is rounded off with a meal.

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[1] Perhaps the instinctive tendencies to make such adjustments as are necessary to secure sleep and excretion are also of sufficient importance in the interpretation of human behaviour as to justify inclusion in such a list.

[2] James, *Principles of Psychology*; Drever, *Instinct in a Man*; Carveth Read, *The Origin of Man*; Burt, *The Young Delinquent*.

[3] Burt, *The Young Delinquent*; Hall, *Psychology and Adolescence*; Davenport, *The Feebly Inherited*.

[4] Trotter, *Instincts of the Herd in Peace and War*; MacCurdy, *Problems in Dynamic Psychology*; Carveth Read, *The Origin of Man*; Tansley, *The New Psychology*; McDougall, *Outline of Psychology*.

[5] A fuller discussion of the instincts in the light of this criterion, and also of Play and the higher human values, will be found in *Instinct and Personality*, by the present writer, now in the press. (Messrs Geo. Allen and Unwin.)

In the peculiar case of the annual migrations of birds and animals the stimulus of the instinct almost certainly lies in seasonal changes, and perhaps also in internal secretions. But the stimulus of the more general wandering tendency is not so obvious. The impulse takes its rise, not from the presentation of some change in environment, but rather from its continued sameness. In man, and apparently also in wandering animals, sameness of environment sooner or later begins to bore and the impulse then arises to seek a change. The finding of change brings satisfaction, and in the end creates satiation. It is not only man that, tired of wandering, turns again home. The wandering animal usually returns, again and again, to its old haunts. The horse that wanders from his stable, as every farmer knows, turns almost invariably in the direction of the place where it was reared, even though it be many miles away; though, on the other hand, they will wander far away from their accustomed feeding ground and from the place of their rearing.

The Wandering impulse, therefore, is one that rises quite independently of the needs of food and sex (though these needs also lead to wandering) and it finds an independent satisfaction. It possesses no peculiar and well-marked emotional tone and therefore McDougall's affective criterion fails to reveal its existence. But the conative criterion here proposed enables us to distinguish it from the other impulses with which it is usually closely interwoven. We are compelled to recognize an independent Wandering instinct; and the value of this may be seen, for example, from the fact that Burt[1] has been led, by his study of delinquency in children, to place an unsatisfied and uncontrolled Wandering instinct among the very common causes of youthful misdemeanour.

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[1] Burt, *The Young Delinquent.*

2. *The Hunting Instinct.*

The Hunting instinct, like Wandering, must have been originally associated with that of nutrition. But some animals will hunt in the absence of the stimulus of the need of food and apparently find a satisfaction in securing a kill even though they do not want to eat their victim. The tiger, notoriously, has a reputation in this respect. The dog, too, manifests the same tendency for, even though domesticated, well fed, and trained not to molest domestic animals, this native instinct will sometimes break the bonds of habit and training; he raids the sheep-walk or poultry-yard, mauling and killing far more than he can eat, just for the sheer delight of hunting. To a similar charge man must also plead guilty.

The Hunting impulse requires as its end the killing of something[1]. Only when that is accomplished is complete satisfaction achieved. As a boy, when I have hunted birds or animals that I have had no intention either to keep or kill, it has nevertheless never been without a pang of regret for something lacking in the completeness of the satisfaction obtained that I have let them go again. Hunting a drag instead of a fox is commonly described as 'tame sport'; so is shooting at mechanical moving targets compared with pigeon shooting. If there are no rabbits to be shot the youth with the pea rifle will destroy beautiful birds that are of no use to him, simply because he finds a satisfaction in it which he does not find in setting up a target to shoot at.

The stimulus of the instinct is, in the main, that complex situation in which hunting is perceived to be possible. This differs for different creatures. The need of food is commonly also an important part of the stimulus, if not the adequate stimulus. But where hunting is engaged in for its own sake the mere occurrence of the hunting situation—the perception of the right kind of living thing in a position where it can be killed by hunting alone—seems sufficient to arouse the impulse. This means that it is the situations under which the instinct has developed that have eventually become its adequate stimuli. In man, mere killing does not satisfy the impulse; the mere opportunity to kill does not rouse it. The impulse is stirred by the opportunity to *hunt*; it is gratified by a kill which is the result of hunting. Even the cat playing with a mouse waits for its victim to run, and often allows it to get some distance away before again manifesting the impulse to chase and seize it. A dog too is usually not excited by a person walking past him; but he is very apt to give chase to a runner. The stimulus of the instinct therefore seems to be as specific as the end.

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[1] Thouless, *Social Psychology*.

3. *The Herd Instinct.*

Passing over those instincts regarding which I find myself in general agreement with McDougall I come now to refer to the Herd instinct, the theory of which has had a chequered career in the psychology of the past two decades. The elementary fact that certain species of animals tend to gather together and move about in groups is undoubted. The question on which there is difference of opinion is as to how much more —if anything more—is involved in the tendency besides this mere gregariousness.

A careful consideration of animal habits will show that the gregarious tendency is much more widespread than at first appears. It is certainly present in man. Our human associations give us a satisfaction of their own altogether apart from the material and intellectual benefits we derive from them. Man abhors solitude. Normally he finds a peculiar emotional delight in congenial society and experiences the painful emotional tone he calls loneliness—a restless dissatisfaction—if long deprived of it. Here then we have a conative tendency which can only find its full and final satisfaction in association with others of the species. Certainly, then, we must recognize a Herd instinct.

The primary stimulus of the instinct is the perception or thought of a group or individuals of that society in which it finds satisfaction. If one dog sees another in the street he needs must run up to him and exchange formalities. If he sees a pack he must join it—if the pack will admit him. A horse let free in a field, seeing other horses on the other side of it, will almost invariably trot over to them even though they are half a mile away. Even when the animal is hungry he usually will not stop to feed until he has joined his fellows. Most of the higher animals, even to the notoriously unsocial cat, betray this tendency to some extent.

The existence of a tendency to herd together being admitted, then, and its stimulus understood, we come to the question whether this instinct ever requires for its satisfaction anything more than mere presence with the herd. McDougall has stoutly maintained that it does not. Trotter, on the other hand, has attributed almost every form of social behaviour directly to Herd instinct. In this, McDougall, I believe, is the nearer to the truth. Some animals, perhaps, such as the Damaraland oxen described by Galton, may demand no more. But this is certainly not true of all. The dog seeks, not merely to find himself present with other dogs, but also to secure their recognition; and he is not satisfied until he obtains it. Horses manifest the same characteristic though in less degree. On approaching a herd unnoticed a horse will commonly whinny to attract attention and he is never satisfied to commence grazing until he has received at least a toss of the head in his direction from some member of the herd.

This feature of the Herd instinct is particularly noticeable in animal pets. It can be no other than Herd instinct that impels an animal to attach itself to, and to follow, a human master, constantly seeking his society, though it is only under artificial conditions that a human being can be thus brought within the scope of an animal's herd, indeed, become a kind of herd leader. Yet this relationship can be established between man and any of the notably gregarious animals; indeed it is only those creatures in which this instinct is strong that show faithfulness and attachment as pets. After all, it is no stranger that the Herd instinct should be thus directed toward a creature of another species than that the Parental instinct should be so modified; and this also is of common occurrence. Not only do ducks mother chickens and cats, puppies; but a hen may gather kittens under her wings, and a bitch may suckle a brood of young pigs. The relation between a man and his dog is therefore quite a legitimate one for the study of the Herd instinct, and it bears striking testimony to the fact that this instinct usually demands for its satisfaction some sign of recognition from other members of the herd.

Further, it must be an approving recognition. The dog that is met with a scolding reveals manifest displeasure and uneasiness. He might be more uneasy if not recognized at all, but his Herd conation is not adequately satisfied until he has received approval. That the same holds good of our human social cravings will be readily agreed and this recognition of the multiple nature of the demand of Herd instinct—for the *presence, recognition* and *approval* of the group—is of very great importance for the understanding of human conduct.

But the demand of Herd instinct is complicated by yet another factor of equal importance. This is the tendency engendered in each individual to give such emotional expression to his impulses as tends to arouse in others conations similar to his own. This, McDougall has rather inaptly called Active Sympathy. It is a tendency to seek for sympathy from the herd and might, I suggest, be better described as the Sympathetic Craving. This demand for sympathy, for fellow-feeling, for the knowledge (in man) that his desires and interests are shared by the others of the group, must be recognized as pointing to a fourth constituent of that complex condition of consciousness in which alone Herd instinct finds full satisfaction. No gregarious animal, least of all man, is content to feel a strong emotion, or conative tendency, and to feel it alone, knowing that the other members of the group do not feel it. That condition is one which divides him from the herd and he is subtly conscious of the difference and therefore dissatisfied. Under such conditions one can feel intensely lonely even in a crowd. The natural tendency is therefore either to express such feelings and thus call upon the group to share them or, if this be not possible, to repress the emotion or tendency. Failing this the individual may become so uneasy in the presence of the group that he leaves it.

This demand for sympathy lies at the basis of private friendship as well as group fellowship. There are many of our interests and desires which we can get no group to share because they are unusual or too intimate. We therefore seek out some congenial soul in whom we can be sure of finding a response and become bound by ties of common interest and purpose to him. Such an individual acquires a value for us above others and this relationship comes to provide the motive basis for much that passes for pure altruism, though it has this definitely egoistic basis.

But though the application of the suggested conative criterion shows Herd instinct to possess a much fuller significance than that conceded by McDougall it yet seems to me that we can find no phenomena that would indicate that this instinct ever impels directly to altruistic action as such. For the true altruistic motive we must look elsewhere. Instances from animal life where one animal or bird cares for an adult mate in distress are to be attributed to the Parental instinct aroused, usually, by the typical and peculiar appeal of the species. Other cases, which cannot be explained in this way, such as the united fighting of a common foe by all the herd when the escape of the majority would be easy, are readily explained by the sympathetic arousing of the appropriate instinct (in this case Pugnacity) in the whole herd through the emotional expressions of one or a few of the members. There is nothing in animal activity therefore to indicate the existence of a native tendency to serve the interests, as such, of the herd.

Turning to the introspective evidence of our own herd impulses we find that they attain full satisfaction when we are able freely to *associate* with the herd, are *recognized* and *approved* by its members, and can *share its emotions and interests*. If any of these four things is lacking there is something wanting in our satisfaction[1]. But we are content with our society and our relations to it so long as these four conditions are fulfilled. *There is no need that we should also be conscious of having in any way served the herd.* If we have served it, consciousness of that fact may heighten our total sense of satisfaction; but if so it is not because the demand of our herd instinct has been more completely met, but because such conduct has helped to fulfil a demand we have made upon the self, a demand of a very different nature from, and one which, I believe, transcends, that of the herd or any other of those instincts which man shares in common with the higher animals.

4. *The Instincts of Display and Domination.*

Having contended for the existence of an approval-seeking tendency as part of the working of the Herd instinct it may be asked what necessity there is for including in the list given above an instinct of Display. The reasons for recognizing the tendency to Display as a separate instinct are two. In the first place it manifests itself apart from the Herd tendency in connection with the Sex instinct. The admiration of the opposite sex is an end sought for its own sake not only by peacocks and stallions but by men and women. In the second place, there is a conation impelling to Display which receives satisfaction from the admiration of a group from which the individual seeks none of the other satisfactions of herd instinct—association, recognition and sharing of interests. A man will display himself before people he hates and despises. It seems necessary therefore to recognize in man (and in some other animals) a separate instinct of Display which is stimulated: (*a*) by members of the opposite sex who also stimulate the Sex instinct of the individual, (*b*) by members of a group other than that of the individual concerned, (*c*) by members of the individual's own group. In each case the response is somewhat different but *the end in which the conation finds satisfaction is always the admiration and approval of those who have stimulated the instinct.* This recognition of a separate instinct of Display must not, however, make us overlook the fact that the approval of one's fellows is also a necessary part of the satisfaction of the Herd instinct.

The instinct of Domination is included by McDougall with Display in the one instinct of Self-assertion and Self-display. But this would seem to be unjustifiable, for the ends sought are entirely different and often contradictory. Under the influence of the impulse to dominate, children may often be observed to do things that they know will bring them the very reverse of admiring approval from their fellows. Introspection, too, shows that the satisfaction found in achieving domination over a number of one's fellows is very different from that obtained by securing their admiring approval; and the former satisfaction is quite independent of that approval if not given, and may even be enhanced by the absence of it. Many a domineering taskmaster has gloried in the fact that he is hated. *The satisfaction of the instinct seems to lie simply in the achievement of leadership and power*; its stimulus is the situation which presents the opportunity to acquire such ascendancy. The instinct must have found its origin mainly in the hunting pack[1]. Every pack requires a leader and it is the instinct of Domination that singles the leader out by creating a competition for leadership. The instinct has its counterpart in that of Subjection which brings it about that those creatures forced to accept a position under domination develop a tendency gladly to accept and to find satisfaction in the leadership of their manifest superior.

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[1] The effectiveness of this complex Herd motive among primitive people is clearly shown by B. Malinowski, *Crime and Custom in Savage Society.*

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[1] Carveth Read, *The Origin of Man.*

(*Manuscript received* 16 *June* 1927.)

THE PSYCHOLOGY OF ROWING

BY P. E. VERNON.

(From the Cambridge Psychological Laboratory.)

1. *Psychological factors in individual rowing* (pp. 317–319).
2. *Affect and suggestion* (pp. 319–321).
3. *The rowing of the group: group self-respect* (pp. 321–325).
4. *The boat club group and the rowing personality* (pp. 325–329).
5. *Possible criticisms* (pp. 329–330).
6. *Summary* (pp. 330–331).

1. PSYCHOLOGICAL FACTORS IN INDIVIDUAL ROWING.

THE intensive study of special groups with a view to correlating part at least of the mass of general theories with the facts of observation seems to be one of the main needs of Social Psychology at the present time. The following is an attempt at a study of the rowing VIII and of a college boat club, both of which seem to afford excellent illustrations of certain theoretical points such as the nature of suggestion. We will first consider the part played by psychological factors in the rowing of the individual and then of the group.

Rowing for the individual is, more or less, a process of continual warfare between the lower or physiological and the higher or psychological functions; it consists almost entirely of the maintenance of hard work and the attainment of style by the utmost efforts of attention and will. Whatever these may be they are, of course, greatly influenced by affective dispositions, so that for the inexperienced oarsman, at any rate, his general mental and emotional attitude in rowing is of more importance than his actual strength and muscular ability. It is absolutely essential that he should be self-confident rather than self-pitying; any ideas of tiredness or desperation, any doubts or fears as to his being able to last out while rowing a course always seem to realize themselves by auto-suggestion, and prevent him from doing the best of which he is capable. There are, of course, physiological reasons for starting off a piece of rowing as hard as possible, but the coaches' advice to their crews to start rowing a course as if they were only going to row for ten strokes is also psychologically sound, since thinking about how much farther they have to go inevitably makes them slower. A highly effective attitude for the oarsman is that of rage against his opponents, probably therefore a vigorous, 'pushing' temperament makes the best rowing type[1].

The effects of coaching on the individual are also mainly mental, especially when he has sufficient experience to know for himself if his movements are more or less correct. The good coach largely aims at establishing the necessary self-confidence by not making rowing appear to him too difficult or painful (the same effect can be illustrated from Industrial Psychology by the well-known tabulation of American census returns[2]). Too much nagging by his coach may result in his becoming 'fed up' and adopting a hopeless outlook, while, on the other hand, no criticisms at all may make him become suspicious or fearful that he is too bad to be kept in the boat; all these attitudes make his rowing much worse. An occasional word of praise, however, operating by suggestion, may make him improve considerably and inspire him to much more effective efforts than before. Similarly when rowing past another boat of his own club that he wishes to impress, or when passing the boat club captain, who may decide what boat he is to be put into, the style and work that he has laboriously and unsuccessfully been trying to attain during the whole outing sometimes seem to come of their own accord.

There are innumerable influences which might be expected to have but little effect on such physiological activities as a man's style and work, but which may distract him considerably and prevent him from doing his best; for example, the cold, the heat, the sun in his eyes, the wind, the rain, splashing, too much lunch, too little sleep the night before, too much mental work during the day, blisters on his hands, an oar handle that seems too short, too thin or too thick, the passing of other boats or of people on the bank; even if these exist only in the imagination they disturb him quite as much as if they were real. The more experience he has and the more he can concentrate severely on the task in hand, the less do such little things matter; but I am mainly concerned not with the exceptionally good oarsman but with the average one who has great difficulty in disregarding them. Worst of all is the fact that every bad point or distraction in one member of the crew makes the boat go worse and therefore lowers the standard of the rest; this lowering, reciprocally, makes the first man still worse, and so on in a vicious circle.

This close interdependence of every individual on the rest of the boat makes it especially profitable to consider the boat, while paddling or rowing, as an organism. Each part of it has to work harmoniously with all the rest, for just as in an animal, if one organ is unhealthy, the vitality of the whole is impaired, so in the boat, if one man slacks, the whole boat feels heavy and dead; again, as in the animal, if one leg muscle or tendon gives way, the equilibrium of the whole is disturbed; so, if one man relaxes the pressure of his feet on the stretcher or brings his oar through the water too high or too low, the balance is lost and the whole boat rolls over to one side. On the other hand, when the boat has really reached the stage of going well, such slight deviations from the normal are immediately compensated for by minute, often unconscious adjustments on the part of the rest of the crew. There is therefore very close interconnection between the members, yet there is but little verbal intercommunication while on the river; indeed, there are few things more objectionable in rowing than the man who tries to coach other members of his own boat; hence the nervous system and circulation of our organism, the boat, are almost entirely supplied by the general feel of it, whether it is light or heavy to pull along, smooth or jerky, well balanced or unsteady, in fact by what is known as the 'run' of the boat.

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[1] It is interesting to note that this has also been found to hold good even of intellectual problems: cf. G. M. Stratton's essay in *Problems of Personality*, 1925, p. 58.

[2] Quoted by Hollingworth and Poffenberger, *Applied Psychology*, 1923, p. 278.

2. AFFECT AND SUGGESTION.

Before going on to the rôle of group psychological factors it would be as well to consider how these observations bear on psychological theory. It is noteworthy that, whenever an attempt is made to apply psychology to everyday life, one is immediately confronted with the problem of the nature of affect, emotions and suggestion. On the whole the most profitable point of view seems to be to regard all varieties of affect as sets, attitudes or configurations of the whole organism, as chemical, muscular, nervous and mental patterns, the organic or physiological part of the pattern being able to induce the whole. Suggestion is then the reinforcement of an idea by an affective disposition which enables it to overcome other mental processes that might be expected to oppose it. When a man is rowing his natural affective state is one of weariness and desperation, hence suggestions to that effect are readily accepted (there may even be processes exactly analogous to the pathogenic autosuggestion so often occurring during the war when hysterical paralysis and the like resulted from unconscious desires to get away from the firing-line at all costs). If however he can establish in himself the proper self-confident and aggressive attitude, or if his determination to row well can overcome his desires to give up, then suggestions such as the occasional word of praise or self-congratulatory autosuggestions are much better able to take effect. There is nothing original in this explanation; suggestion is used here in the same sense as that given by Prideaux[1] and Flower[2], namely, indirect mental influence in contradistinction to direct mental influence or logical persuasion, also in the sense given by Hart[3], namely, a variety of 'complex thinking.'

Having thus defined it we may proceed to another most important aspect of it, its dependence on the relationship between suggester and suggestee. McDougall and Freud in their efforts to determine its limits and its nature adequately seem to have confined themselves entirely to the 'prestige' type. McDougall assumes that when human beings respond to one another by suggestion there must be relations of dominance and submissiveness between them. This prestige relationship is, in effect, the same as the transference relationship of Freud[4]; this is to be regarded as an atavistic survival whereby the libido is fixated on the suggester in the same way as it used to be fixated on the leader of the primitive horde; hence Freud assumes that in a group the libido of all members is fixated on the leader or on an abstract idea that takes his place. Similarly Trotter gives to the herd instinct the same prestige form, suggestions being accepted because they are identified with the voice of the society. But none of these theories account for the suggestibility of members of a group towards one another, where the social relationship is one of equality or primitive comradeship[5]. This is however the characteristic feature of the rowing VIII, for neither does the prestige relationship hold between members of the crew, nor, without an unnecessary stretch of the imagination, a libidinous or transference relationship. As in an organism an hypertrophied or dominating organ is a sign of ill-health, not of increased efficiency, so in the boat there is no leader in the ordinary sense of the word. Stroke is supposed to determine the rate of striking, when spurts shall be made, and so on; actually he is quite unable to do so unless all the members of the crew are so closely intercoordinated with one another that each one quite naturally seems to want to do what all the rest do. It is obvious by now that Rivers' account of suggestion fits the VIII much better than McDougall's or Freud's; he defined it as "that aspect of the gregarious instinct whereby the mind of one member of a group of animals or human beings acts upon another or others unwittingly to produce in both or all a common content, or a content so similar that both or all act with complete harmony towards some common end[1]." Such suggestion is of the more primitive 'hint' type, where, instead of 'communicated propositions' being 'accepted with conviction' we have the almost 'unwitting' acceptance of unformulated suggestions and hints. It follows from the physiological theory of affect used here that not only is the mental content made similar by hint suggestion, but also bodily movements and postures; Rivers calls this aspect of the process mimesis[2]. He gives one or two well-known illustrations drawn from primitive peoples and animals, but surely it is unnecessary to look for material so far from home, since groups of the primitive type can be found and studied in modern society, the rowing group being a good example; such groups carry the additional advantage that the observer can enter completely into the group life, which cannot really be possible in the observation of savages.

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[1] E. Prideaux, This *Journal*, 1920, x, pp. 228–241.

[2] J. C. Flower, *Brit. Journ. Med. Psych.* 1923, III, p. 42.

[3] B. Hart, *Psychopathology*, 1927.

[4] S. Freud, *Group Psychology and the Analysis of the Ego*, 1922, chap. IV.

[5] F. C. Bartlett, *Psychology and Primitive Culture*, 1923, p. 36.

It is not at all difficult to reconcile the various types and theories of suggestion if we regard it as indirect mental influence operating through affective dispositions. McDougall appeals solely to the ego instinctive tendencies, Freud to the sexual, Rivers to the gregarious; in practice it cannot be confined to any one tendency or set of tendencies. The autosuggestion that operates in the rowing of the individual obviously depends on no one of these alone; suggestions that improve his rowing do so through his self-assertive and fighting dispositions and sometimes possibly through fear of the coach or of being turned out of the boat. Suggestions that make his rowing worse do so chiefly through his conscious or unconscious desires to give up, his weariness, the revolt of the body against the work that is being imposed upon it. That fatigue and what are commonly accepted as instincts can both operate in the same fashion is a point strongly in favour of the general theory of affect outlined above.

Having cleared up this point we may pass on to the consideration of group psychological factors in rowing.

3. THE ROWING OF THE GROUP: GROUP SELF-RESPECT.

It was stated above that one man can make the boat go badly, but that when the boat is going well the rest of the crew can compensate for any deviations from the normal. What then constitutes the difference between going well and going badly? In practice this distinction is most marked and obvious; in the early stages of training every man feels exceedingly uncomfortable, the boat never seems to him to run level, no one in the boat but himself appears to be doing any work; he may even get so tired and 'fed up' as to work only as a painful duty or to avoid the coach's cursing. After a few days or weeks of practice, however, varying with the amount of experience of the members of the crew, the boat fairly suddenly begins to go well; every man enjoys working and does not need any urging from the coach; the boat runs level and does not feel dead or heavy. There are, of course, frequent relapses, the beginning or end of an outing, for example, are usually worse than the middle part; the stage of going well may scarcely be attained at all, or the boat may remain in a rut without improving for weeks on end. This is often known as being 'stale'; just as the mental attitude of the individual, staleness, an attitude of the group, should be regarded as a total set or configuration that may be induced either by its physiological or its psychological aspect. With its accompanying mental effects it may readily be produced by the natural 'physiological' plateaus that always occur in learning. But, as we shall see, its physiological side very often results from purely psychological stimuli. For in the individual it was shown that, though muscular ability and strength may be the ultimate determinants of his goodness at rowing, yet it is largely on temperamental and psychological factors, especially self-confidence, that his style and effective work depend; similarly in the whole boat the ultimate aim is complete synchronization of the movements of every member of the crew, but this necessitates a most important group psychological factor which, for lack of a better name, I will call group self-respect[1]. Staleness, then, in its psychological aspect, is depression of group self-respect, or loss of confidence of the group in itself. The psychological difference between the 'good' boat and the 'bad' one also follows naturally from the general theory of affect that I have adopted; on this pleasure is regarded as the mental accompaniment of harmonious working of several reaction tendencies and physiological activities of the organism, and unpleasure as the result of a conflict between such tendencies and activities. Thus, when the individual can overcome the many distractions and hindrances and row a few perfect strokes the most intense pleasure results, however hard the work may be; similarly in the group when each member has learnt to cooperate harmoniously with all the rest the outings are enjoyed; without such harmony the outings are unpleasurable, very tiring and disheartening, and the boat is still in the bad stage.

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[1] W. H. R. RIVERS, *Instinct and the Unconscious*, 1920, p. 94.

[2] *Op. cit.* p. 92.

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[1] Morale might perhaps be a more convenient term, but group self-respect will be seen to possess many characteristics that cannot be ascribed to morale in its ordinary sense; moreover, the organization of the army and other groups in which morale may be found is quite unlike the organization of the VIII or the boat club.

Group self-respect is the self-regarding sentiment, using McDougall's terminology, of the crew for itself as a whole; it is more than the mere sum of the individual sentiments on account of the reciprocal reinforcement, mentioned above, of every attitude in one member of the group by all the other members. It is, of course, quite unnecessary to resort here to a group mind; it has already been pointed out that the nervous system, the medium of communication between different parts, is mainly the run of the boat, and in no sense is it necessary to assume any kind of mental telepathy. Like most of man's affective dispositions, group self-respect largely operates unconsciously; in fact, any attempt to produce it by talking about it seems to lead to the opposite effect, the fears and doubts that the crew try to repress being more powerful than their conscious hopes and resolutions. One finds innumerable examples of its operation in rowing; every coach knows that neither the individual usefulness of each member of his crew, nor the amount of work done in practice, are true guides as to how successful the boat will eventually be; any racing or regatta affords many examples of crews that are good on paper failing badly owing to their being in an unfavourable psychological condition. Group self-respect chiefly rises and falls with the crew's ideas of the probable success of their boat; hence a good coach by means of convincing optimism can elevate it considerably, also a lucky win on the first day of a series of races may improve it to such an extent that the boat's chances of being successful again on the next day are enormously increased.

Side-by-side racing between boats of the same club illustrate the principle strikingly. Here the lower of the two boats (*i.e.* the one with the worse men in it) is expected to be beaten, its group self-respect therefore will not suffer; the better of the two is expected to win, hence if it does so its group self-respect will not be raised, but it fears that its self-esteem will be lowered by losing, and this fear, by suggestion, realizes itself, so that it is found in practice that the lower boat almost invariably does win, although, if the two boats had rowed for the same distance separately, the lower one would have taken several seconds longer than the upper; for as soon as the lower one finds that it is not losing it has an immense access of self-confidence and esteem and so goes better and better, while the upper one, owing to its lack of self-confidence, goes worse and worse.

From many observations I should estimate this psychological factor of lack or access of group self-respect as making a difference in speed of a length or more a minute. In ordinary racing between different clubs the principle does not hold so well, since each boat tends much more to row its own race at its own speed without regard to its opponent, yet a lead obtained early in the race undoubtedly does give a quite disproportionate advantage. Some strokes say that they wish the crews they are stroking could be made to row blindfold, since seeing the position of their opponents detracts so strongly from their rowing. One is struck by the curious anomaly that in a running race, for example, the individual who loses the lead may often run the better for being behind, but in the rowing race the boat that loses the lead aften rows much worse than usual; the difference must lie in the fact that in the first case we are dealing with an individual, in the second with a group the members of which have to be highly suggestible towards one another and so are more suggestible generally; as in all groups their higher thinking is inhibited, so that it is much more difficult for the boat to row its own race than for the individual to run his.

The coach must be even more careful in coaching the VIII than the individual, since group self-confidence so easily succumbs to pessimistic group suggestions and autosuggestions. If the boat has been beaten by a lower one in a side-by-side race, as just described, it is fatal for him to say that only the lower one was doing any work; the last shreds of self-respect are then likely to be lost. If he nags at the crew continually for hard work before the boat has begun to go well enough for effective hard work to be possible the crew quickly gets 'fed up' and sulks, the boat correspondingly keeps on going badly. On the other hand, if he is too optimistic and adulatory the boat is likely to improve too fast; then doubts and fears, which may or may not be consciously acknowledged, develop very easily and, again by suggestion, depress the group self-respect. The chief necessity is that the crew should have great confidence in him, else they will be unable to build up the necessary confidence in themselves; thus he must have a good coaching and rowing reputation and must, in general, be sufficiently tyrannical to be obeyed unquestioningly. But perhaps even more effective as a coach is a leader of the persuasive type who is so familiar with his men that he is himself a member of the group; in this case the confidence of the crew in him is much more complete than if it were established by mere dominance. For very inexperienced oarsmen this type of coaching is not possible, since it is not easy for them to form a homogeneous group at all, much less one that includes the coach too. It should also be noted here that the crew must have as much confidence in their cox's powers as in their own; if in a bumping race they doubt his capacity to steer so as to hit the boat in front, they are very unlikely to be able to make the final spurt necessary for getting the bump[1].

4. THE BOAT CLUB GROUP AND THE ROWING PERSONALITY.

Not only good coaching is essential for the development of group self-respect but also general mutual friendliness between all members of the crew; to row well together they must be in very close *rapport* with one another. This must, of course, be largely built up off the river, in the boathouse, for example; in a college, however, the group life of the boat club is extremely favourable to its development and is therefore of great psychological interest. Both the suggestibility of each man towards the rest of the crew and the group self-respect are intensified by it, for the comprehensive club-group-self-respect includes all the boat-group-self-respects; hence success or failure of one boat of the club raises or depresses the self-respect of every other boat to a certain extent. An examination of the racing at Cambridge for the last few years shows that, almost always, boat clubs as a whole are successful only when their lowest boats, *i.e.* those that race first on each day, do well. If, however, the first boat to race does badly, the suggestion that the club as a whole will do badly is very prone to take effect.

To develop the necessary intimacy there is constant association together of the members of the crew and of the whole club, the average rowing undergraduate being in the company of other members of his club for half the day or more; even when he attempts to work he often does so together with several others of his species. The boat club sits at a table by itself at dinner and rejoices in its exclusiveness and its superiority to outsiders. When in training it eats special foods for which there can be very little physiological justification. The highly coloured blazers, characteristic symbols of modern groups, originated last century in the particular college boat club with which I am concerned.

Participation in the group life becomes fuller as the races draw near; whole crews then frequently have tea together, in this way bolstering up their group self-respect. Often only about half a dozen men really represent the boat club at the beginning of training for the races, but by the time the races are reached everyone rowing has become a genuine member of the group.

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[1] Racing at Cambridge and Oxford is not of the usual side-by-side variety, but all the boats race behind one another, each one trying to catch up and bump the boat in front of it, and, at the same time, to keep away from the boat behind it.

A further corollary of the principle that group self-respect is essential for a boat to be successful is that each member of the club must fit in well with all the others; to some extent, therefore, the personalities of all the crew or club must be similar. Hence it frequently happens that a man who is at all abnormal as compared with the rest, or one who maintains his individuality instead of subordinating it to the group life, is a wet blanket or a Jonah in whatever crew he is put into, so that development of a really effective group spirit is impossible. I would even go so far as to say that the presence of certain men in the boat, however good their style and work, limits its greatest chances of success to one or two bumps out of the four possible, that is to say, such men are temperamentally incapable of being in a boat that wins all its races or makes four bumps; yet in another club, consisting of men more of their own type with whom they accord better, they are likely to be quite successful. Usually it is found in a boat club that the less suitable types drop out soon after they take up rowing, not from lack of strength or skill but from some incompatibility of personality that prevents them from harmonizing well with the rest of the group. Similarly many who are no use at all at most forms of sport often turn out to be quite good at rowing in an VIII, and this is largely because they possess the necessary social qualities. Naturally it is seldom that all members of a crew even approximate to the ideal of complete harmony, but the nearer they are to it the stronger is their group self-respect likely to be and the better will they probably do in the races. Thus it is significant that the Cambridge College whose boat club has been the most successful of any in recent years is the only one to which the entrance is limited to a particular type of man, so that all the personalities of the members of its club have an excellent chance of harmonizing well.

The rowing personality therefore offers the psychologist an interesting field of investigation. Certain of its traits may be mentioned here as they will be of use in our study of the group. So far we have seen that high suggestibility and a vigorous self-confident temperament are desirable. Next, almost certainly, the average rowing personality is of the extraverted type; the more introverted or neurotic person who starts rowing drops out very quickly. To my mind the chief reason why the psychology of rowing is such a little discussed subject is that the true rowing man is not interested in it; the 'subjectively minded' man is the one who worries, and who tends to row much worse for doing so owing to the ubiquitous operation of suggestion. The other is therefore quite right in trying to row and coach on the basis of plain commonsense and his own experience; actually these usually turn out to be thoroughly sound, psychologically. Again, according to Prideaux[1], extraverts or those of 'sanguine temperament' are much more suggestible than introverts, hence the latter harmonize with the rest of the crew much less easily than the former.

Possibly another aspect of the same trait is the character of their imagery; when they coach they use highly 'picturesque' language and are continually drawing analogies and descriptions from other walks of life, these being much more effective than logical scientific or psychological arguments. Examples of such analogies are: "Balance the boat like a bicycle by keeping her running quickly and smoothly," "Reach up with the hands just before the blade of the oar enters the water as though offering someone a cup of tea," "Jump up from the stretcher," and so on.

The attractions of rowing also throw some light on the nature of the group. The disadvantages and discomforts are too many and obvious to need enumerating; even the pleasures of success are, for the average oarsman, few and far between, and he is continually being put into lower boats than he had hoped for. However, owing to the nature of the club group spirit, if only a small proportion of the boats do well the whole club celebrates it with jubilation, the failures of the unsuccessful ones thus being compensated for. A considerable attraction, in university rowing perhaps the chief one, is the satisfaction of gregarious tendencies, but, as the essential regressiveness of group life may be a drawback rather than an advantage to the higher mentalities, one must conclude that the drudgery, the exercise and the healthiness often constitute the chief reason; the victory of mind over body is highly pleasurable. There is also a great deal of aesthetic pleasure to be obtained from the sight and performance of athletic activities, for physiologically correct movements and postures are the most beautiful, and the better the individual rows the more physiologically correct his movements become, so that watching a really good boat row, or rowing in it oneself, affords an aesthetic delight that can only be compared to that obtained from the perfect performance of, say, a string quartet. If the boat one is watching belongs to one's own club the pleasure is particularly enhanced by the sense that one is part proprietor and creator of this sight, since one is a member of the group and so has contributed oneself to the group spirit that lies behind its attainments.

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[1] *Op. cit.* p. 230.

Finally, reasons such as the ideal of obtaining a 'Blue,' the sublimation of sexual tendencies, and the moral education may play a small part, but it is justifiable to assume that, for the average oarsman, his motives may be traced back very simply to his gregarious instinctive tendencies and either his sexual or ego ones or both.

All this leads us to the more purely psychological characteristics of the group; it seems to indicate that the society is of a very primitive nature, firstly because its *raison d'être* is simple. Secondly, there is very little organization as compared with that of a group such as an army, since few members are likely to be prominent for more than a year or two. Bow in the fifth boat feels himself almost as integral a part of the club as stroke of the first boat. If there is a leader at all he is more of the persuasive than the dominant type, one who 'expresses rather than impresses' the group[1]; thus the social relationship between all members is, or should be so far as possible, one of equality, not of assertion and submission; this also becomes more marked as the races approach.

The boat club is much less plastered over with tradition and sentiment than most complex modern groups, partly owing to the continuous change of the individuals that compose it, for the average generation is three years or less in duration. Every member is to some extent proud of and conscious of the group, but this refers more to the present group than to its former history, and is a sign of group self-respect rather than of the influence of the past.

Possibly a further sign of the primitive type of group is that a good or bad leader, while still conforming to the persuasive type, can have a considerable direct influence on the boat club. One of the characteristics of modern complex society seems to be that the individual has scarcely any direct influence on the group owing to the distorting effects of the affective forces that bind it together, whilst the simpler the society the less distortion would one expect.

In accordance with the scheme of this enquiry, namely, the collection of many facts of observation and then considering them to decide whether they do or do not bear out the generally accepted theories, I should like very tentatively to draw attention to one more characteristic of group life, one that has perhaps scarcely been emphasized sufficiently before. It cannot but strike one in a community like a college or a school what a prominent part is played in all kinds of social activities and organizations by those who row; in fact there are very few of such activities that are not run by members of some clearly defined group centred round a strenuous sport such as rowing, running or football. Nor are such men, as is often supposed, deficient with regard to outside interests such as intellectual, aesthetic, political or religious ones (the first of these can be proved by treating examination results quantitatively). Moreover, such activities are undoubtedly favoured by the group life and athletic interests; many unbiassed observers would hold that the average man would not pursue most of them so vigorously if he did not row; the peculiar gregarious self-satisfaction produces, as it were, superfluous energy that overflows into these activities. Now most contemporary psychologists, and especially medical ones, seem to assume that all man's activities are subserved by a general, undifferentiated nervous and mental energy; for example, the libido of Freud and Jung, the neurin of McDougall, possibly also the attention of Ward and the vigilance of Head, etc. This is limited in quantity, so that the more spent on one activity, the less there is for the others. No doubt what I have described could be explained away on the usual theories, but surely a more careful consideration of the facts would demonstrate any number of similar cases where, apparently, the more energy a man expends on group life and on strenuous sports, the more he has available for other interests; it is a truism that "if you want anything done give it to a busy man."

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[1] Cf. Bartlett, *op. cit.* p. 79.

5. POSSIBLE CRITICISMS.

Before concluding I must point out what will be obvious to any rowing men who may happen to read this, but which is also true of most applied psychology, that I may have tended to exaggerate throughout, that the story is, in fact, too good to be true. The chief modification that I ought to make is that psychological factors and autosuggestion play a much less important part in the rowing of the thoroughly good and experienced oarsman. The best boats of different clubs are, of course, much more nearly equal than the worse ones since the ultimate physiological limits are much more nearly reached by each man. Good oarsmen can very likely remember boats that were successful in which they were not friendly with the whole of the crew, or races that they won in spite of losing the lead at the beginning. Yet, for the reason already stated that the best rowing men are not and should not be interested in psychology, they are just as likely to under-estimate the importance of psychological factors as I am to over-emphasize them. It is, of course, most difficult to distinguish how far the speed or slowness of a boat is due to the actual capacity of each man in it or to the amount of work done in training, and how far to psychological conditions; it is not my object to say that the latter play a greater part than the former, or *vice versa*, but to show that both are equally valuable aspects of the same process.

I should also point out that I know very little of rowing outside the Universities and public schools; possibly half or more of the total oarsmen in England do not live in such groups as I have described, and it is not easy to say how much or little difference this makes. From observations at regattas, however, one is justified in concluding that, psychologically, they are all very similar. But this limitation also bears little weight, since the paper is largely based on the criticisms of other rowing men, whose help I should like gratefully to acknowledge here.

6. SUMMARY.

The main conclusions may now be summarized as follows:

1. In Social Psychology the study of special groups is needed to throw light on many general theoretical points; this paper is concerned with the groups constituted by a rowing VIII and a college boat club; they are much more convenient sources of material than primitive peoples.

2. In the rowing of the individual, psychological factors such as his mental attitude are of equal or greater importance than his strength and muscular skill; his style and work are greatly influenced, often unconsciously, by autosuggestion. This applies less to thoroughly experienced oarsmen.

3. The nature of suggestion, namely, indirect mental influence operating through affective dispositions, is demonstrated in individual rowing. In the rowing of the group 'hint' rather than 'prestige' suggestion is of primary importance, since the relationship between members of a crew is one of equality, not of dominance and submission.

4. The VIII may profitably be regarded as an organism owing to the reciprocal reinforcement of every attitude in one member by all the others through the medium of the run of the boat.

5. For a boat to go well and win its races there must be developed both a close physiological and psychological interconnection between members of the crew; the former consists in harmonious synchronization of the movements of every member, the latter is a sentiment, group self-respect. Each depends upon and conditions the other.

6. The level of group self-respect rises and falls chiefly with the crew's ideas of the probable success and failure of their boat. It is greatly affected by coaching.

7. The group life of the boat club and the rowing personality are well adapted to the development of the necessary *rapport* between members of the crew. The boat club is a society of a very primitive type.

8. The observations also support a physiological type of theory of affect and do not support the concept of a general undifferentiated nervous or mental energy.

(*Manuscript received* 20 *September* 1927.)

INFLUENCES OF CONATION ON COGNITION[1]. PART II

By E. H. WILD.

(*From the Psychological Laboratory, University College, London.*)

I. *Introduction* (pp. 332–333).
II. *The Test Material* (pp. 333–334).
III. *Experimental Procedure in Training Colleges* (pp. 334–336).
IV. *Experimental Procedure in Schools* (pp. 336–338).
V. *Qualitative Experiments for Introspections* (p. 338).
VI. *Analysis of the Quantitative Results* (pp. 339–348).
VII. *Summary and Conclusions* (pp. 348–353).
VIII. *Mathematical Formulae used* (pp. 353–354).

I. INTRODUCTION.

IN a previous paper (25) the present writer summarized most of the experiments that had already been carried out on this problem, and analysed them on the basis of the Hormic Theory of Action and in the light of Spearman's Principles of Cognition.

For an understanding of the present discussion, it will be necessary for the reader to refer to the relevant works by McDougall and Spearman (10, 11, 19).

The present article must be regarded as a direct continuation of the earlier paper.

The object of this research was to discover what effects were produced on various mental processes, when these processes occurred with different degrees of conation. The experimental part of the research may be discussed under four heads: (i) the choice of test material; (ii) qualitative experiments to obtain introspections from psychologists; (iii) quantitative experiments in which the tests were performed with different degrees of conation by Training College students; and (iv) quantitative experiments similar to the last with schoolboys.

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[1] This paper is based on the second part of a thesis approved for the Degree of Doctor of Philosophy in the University of London. A paper on Part I was published in this *Journal*, XVIII, 1927, 147–67.

Throughout this paper, the necessity for brevity makes it unavoidable that much illuminative and explanatory detail of procedure and analysis be omitted.

II. THE TEST MATERIAL.

In selecting material for the tests an attempt was made to obtain a variety of common intelligence-test forms; to choose tests which required different amounts of conation for their successful performance; to have some of those tests requiring great conation comparable in form with some of the tests requiring little conation; to choose the tests such that a set of them could be easily given to a group within a period of an hour; and to obtain tests which would provide a wide survey of cognitive processes as analysed by Spearman (19).

After practice the writer could adopt the attitudes of maximal and minimal conation readily (cf. Smith and McDougall (18), cited also in previous article (25)). Then a large collection of standard mental tests was worked through, each being done under the two attitudes, and careful introspections on the effects of the attitudes being recorded. Thus the tests were graded into three groups according to the amount of conation, maximal, moderate, and minimal, which proved most successful for their performance. From the tests which showed the effects most strikingly a set of nine was finally chosen to fulfil the other aims mentioned above. This selection was confirmed by further introspections with regard to the amount of conation required in their performance.

The tests in a set comprised the following nine test forms:

Test 1. *Simple Cancellation.* To cross out every 'e' say, the letters being arranged in groups of four to make the test comparable in form with Test 9.

Test 2. *Same or Opposites.* To write 's' or 'd' after pairs of words according as the words of a pair had very nearly the same or very nearly the opposite meaning.

Test 3. *Instructions.* To do simple tasks dependent on understanding complex instructions, *e.g.* "If the fourth word to right of the second word in this sentence has more letters than the thirteenth word in the sentence write 'less' here, etc."

Test 4. *Facts, Sentence Completion.* A test depending on past information, in which the chosen completion word had to be underlined, *e.g.* "The apple is a kind of... bush, shrub, tree, fruit."

Test 5. *Multiplication.* In which five-figure numbers had to be multiplied by 6, 7, 8, or 9.

Test 6. *Inferences.* In which, from given premises, a conclusion had to be deduced and the correct word underlined, *e.g.* "If Jack can jump higher than David, and higher than William, and not as high as Arthur, who can jump the highest?

David? Jack? Arthur? William?"

Test 7. *Number-Checking*. Pairs of three-figure numbers had to be checked as same or different, *e.g.* 756 – 765...d; 398 – 398...s; 593 – 672...d.

Test 8. *Sentence Completion*. A test depending on relation-eduction, similar in form to Test 4, *e.g.*

"He drained the glass because he was so...tired, thirsty, angry, hungry."

Test 9. *Complex Cancellation*. This is an adaptation of a test used by Dr J. J. Strasheim, by which relation-eduction is introduced into cancellation tests.

"In this test you are required to cross out *pairs* of d's, e's and f's, but *only* under the following conditions:

"Cross out a pair of d's when the next *pair* of letters is a *pair* of e's."
"Cross out a pair of e's when the next *pair* of letters is a *pair* of f's."
"Cross out a pair of f's when the next *pair* of letters is a *pair* of g's."

"Example:

"hy~~ff~~ trgg yyrd dtt~~e~~ ~~e~~trf t~~ff~~g ggn~~d~~ ~~d~~eeg yrgg teeg, etc."

"Two of the same letter constitute a pair when they come next to each other with no letter coming between. A spacing between two of the same letter does not interfere with them as constituting a pair."

These nine tests according to the writer's introspections fall into the three groups required. Tests 1, 4 and 7 can be done easily with very little conation; Tests 2, 5 and 8 can be done best with moderate conation; and Tests 3, 6 and 9 can be done only with great conation. This was corroborated by the introspections of the qualitative experiments. The similarity in form of Tests 1 and 9, of Tests 4 and 8, and of Tests 2 and 7 made these tests more easily comparable.

III. EXPERIMENTAL PROCEDURE IN TRAINING COLLEGES.

A. The Conative Attitudes.

The experiments in four Training Colleges were intended to investigate the effects of the conative attitude on cognition, when the conation was directed (i) to rate, and (ii) to quality.

Each group of students did four sets of tests, each set under different conative conditions. Instructions for the four conative conditions were carefully compiled and the appropriate instructions were read out to the group on the occasion of each experiment. The four attitude experiments for each group may be briefly described thus:

(i) Tr or Tense-rate: in which maximal conation was used, directed to rate, *i.e.* to the quantity of the work done.

(ii) Sr or Slack-rate: in which minimal conation was used under the same external conditions (time allowed, etc.) as in (i).

(iii) Tq or Tense-quality: in which maximal conation was used and directed to the quality of the work done.

(iv) Sq or Slack-quality: in which minimal conation was used, under the same external conditions (unlimited time, etc.) as in (iii).

In the Rate experiments (Tr and Sr), each test was too long to be completed in the time allowed for it, and hence both the rate and the quality of the performance could be measured. In the Quality experiments, Tq and Sq, the tests could be completed easily in the time allowed, no time signals being given, and all other incentives to speed being avoided so far as possible; here only the quality of the performance could be measured.

B. Method of Inducing the Attitudes.

Much time was given to the search for some objective method of ensuring that the subjects adopted the required conative attitude, but without success. An account has already been given (25) of the works of Morgan (14, 15), Munsterberg and Kozaki (16), Hamlin (8), Cassell and Dallenbach (2), Evans (6), Todd (23) and Tinker (22), where distractors were found to act as a spur or incentive to greater effort in apprehension and motor-performance experiments; hence it was not to be expected that distractors will produce a lowering of conation for the 'slack' attitudes in the experiments of this research. Incentives for inducing the 'tense' attitudes produce effects which are difficult to interpret or control or measure accurately, as is pointed out by Robinson (17).

The objective measurement of the effort, which is a complementary problem, does not yet seem to be even approaching solution, though some attempts have been made by H. T. Moore (12), Moore and Gilliland (13), Morgan (14, 15), Dodge (5), Thorndike (21) and Wells (24). A discussion of these will be found in the writer's former article (25).

In this research then subjective methods of inducing the conative attitudes were employed. The experimenter used his personal influence to induce the required attitude in the subjects and the responsibility rested largely with the subject in a voluntary adoption of the attitude.

C. Sequence of the Conative Attitudes in the Experiments.

By devising a cyclic order for this sequence, the results could be manipulated to show differences due only to conative changes and not to extraneous factors. The cyclic order devised is shown in Table I.

Hence it is seen (cf. Table II, for schools, below) that if all the Tq results are collected and the Mean found, and if the Mean for all the Tq is compared with the Mean for all the Sq results, the same influences of practice, of 'material' differences, of group differences, and of sequence of the attitudes enter into each Mean; hence any difference between these Means will be due to conation. So also with the Tr and Sr Means. By keeping the nine tests in the same order in all the sets of tests, the influence of one kind of test on the next was kept constant throughout and hence was eliminated in dealing with the differences between the Means.

TABLE I.

College	First experiment	Second experiment	Third experiment	Fourth experiment
A	Tq p_1 a A	Tr p_2 b A	Sq p_3 c A	Sr p_4 d A
B	Tr p_1 d B	Sr p_2 b B	Tq p_3 a B	Sq p_4 c B
C	Sq p_1 a C	Tq p_2 c C	Sr p_3 d C	Tr p_4 b C
D	Sr p_1 b D	Sq p_2 a D	Tr p_3 d D	Tq p_4 c D

where Tq, Tr, Sq, Sr have been explained above;

p_1, p_2, p_3, p_4 indicate degree of practice reached in the course of the experiments;

A, B, C, D represent the four groups of subjects, each group from a different college;

a, b, c, d represent the four sets of tests and the different material comprising the four sets respectively.

[Thus the actual words (represented by 'a') in the opposites test of the set of tests 'a' were not the same as the actual words (represented by 'b') in the opposites test of another set of tests 'b.']

'a' and 'c' were short (q) tests and 'b' and 'd' were long (r) tests.

D. Arrangement of the Test Material.

Each test was typed on a separate sheet of paper, the sheets being pinned together, with the tests in order from Test 1 to Test 9. On the bottom of each sheet were the instructions for the next test (on the succeeding sheet) with examples illustrating that test. In the case of the Rate tests, having read the instructions at the bottom of the sheet, the subjects turned the sheet to do the next test only when the signal to begin was given. In doing the nine tests in order from Test 1 to Test 9, the subjects did a test requiring little conation, then one requiring moderate conation, and then one needing great conation, this sequence being repeated with the remaining six tests.

IV. EXPERIMENTAL PROCEDURE IN SCHOOLS.

A. The Conative Attitudes.

The experiments in four schools were conducted by the class-teacher, as he was more likely to induce the required conative attitudes in the children than would a stranger. Chance effects of varied interpretation of the experiments by the teachers were avoided by giving a full personal explanation to each teacher concerned, and supplying a detailed list of instructions to each. These were so carefully compiled that it was impossible for them to make any variations in the experimental procedure. *All* instructions to the subjects had to be given ***verbatim*** from typed copies. Thus conditions were standardized to be exactly the same for all the school groups, even though a different teacher conducted all the experiments in each school.

All the tests in the schools were Rate tests, *i.e.* too long to be completed in the limited time allowed for each test, thus allowing both the rate and the quality of performance to be measured.

Four conative attitudes were required to be adopted; they can be described here only very briefly as:

(i) N—the normal attitude: in which the subjects worked as they found most natural.

(ii) S—the 'slack' attitude: in which they had to use minimal effort without worrying about the results.

(iii) Th—an effortful attitude: in which they had to try as hard as possible to do their best.

(iv) Ti—with money incentive: in which prizes were offered for best scores and greatest improvement.

As in the Training Colleges, the subjective method was used to induce the first three conative attitudes; rivalry enters as an incentive in the Th. Attitude, and in Ti an appeal is made to rivalry and such tendencies as the possession of a small money prize would satisfy. These appeals vary with the individual and accurate estimations of the effort induced by these appeals is impossible. The experiments aimed then at getting general large differences in effort and discovering the effect on the cognitive output by statistical analysis.

B. *Sequence of the Conative Attitudes.*

For purposes of analysis, it was necessary here as in the colleges to devise a cyclic order for the experiments, and this required a group of boys from each of four schools. Each group did four sets of tests, each set under a different attitude, at intervals of a week. The order of the conative attitudes in the various schools occurred in the order given by p_1, p_2, p_3, p_4, and may be found from Table II. (Cf. Table I, for the colleges, above.)

TABLE II.

School	N	S	Th	Ti
E	N p_1 e E	S p_2 f E	Th p_3 g E	Ti p_4 h E
F	N p_3 h F	S p_1 g F	Th p_4 f F	Ti p_2 e F
G	N p_2 g G	S p_4 e G	Th p_1 h G	Ti p_3 f G
H	N p_4 f H	S p_3 h H	Th p_2 e H	Ti p_1 g H

Attitude-Means:

$$\mathrm{N}\left\{\begin{array}{cccccccc} p_1 & & p_2 & & p_3 & & p_4 & \\ & e & & f & & g & & h \\ E & & F & & G & & H & \end{array}\right\}, \qquad \mathrm{S}\left\{\begin{array}{cccccccc} p_1 & & p_2 & & p_3 & & p_4 & \\ & e & & f & & g & & h \\ E & & F & & G & & H & \end{array}\right\},$$

$$\mathrm{Th}\left\{\begin{array}{cccccccc} p_1 & & p_2 & & p_3 & & p_4 & \\ & e & & f & & g & & h \\ E & & F & & G & & H & \end{array}\right\}, \qquad \mathrm{Ti}\left\{\begin{array}{cccccccc} p_1 & & p_2 & & p_3 & & p_4 & \\ & e & & f & & g & & h \\ E & & F & & G & & H & \end{array}\right\},$$

where N, S, Th, Ti represent the attitudes explained above;

p_1, p_2, p_3, p_4 refer to practice effects;
E, F, G, H represent the four school-groups, each group from a different school;
e, f, g, h represent the four sets of tests and the respective material comprising these four sets.

[Thus the actual sentences of Test 4 in set 'e' were not the same as the actual sentences of Test 4 in set 'f,' or set 'g' or set 'h'; though Test 4 (Facts) was the same in form and order of difficulty in all the sets, 'e,' 'f,' 'g,' and 'h.']

As before the Attitude-Means (as indicated in the last line of Table II) include the same influences of practice, of 'material differences,' of group differences, and of sequence, so that any difference between the means of N, S, Th or Ti will be due to conation. Whereas Tq and Sq could not be compared with Tr and Sr in the colleges owing to 'material differences,' every Attitude-Mean in the schools could be compared with every other Attitude-Mean. The test material was arranged in the same way as for the colleges and the order of the tests in a set was also the same.

The subjects in the schools were given half-an-hour's practice in the form of the tests on each of the two days preceding the first experiment, and ten minutes' preliminary practice immediately before each of the four experiments.

V. QUALITATIVE EXPERIMENTS FOR INTROSPECTIONS.

Before carrying out the experiments with the colleges and schools, the tests were performed by six psychologists and detailed introspections obtained, notably from Dr F. Aveling and Mr J. Flugel. They did the tests (i) with minimal effort, (ii) with maximal effort directed to quality, and (iii) with maximal effort directed to rate. The experiments were directed to introspective comparisons between the mental processes involved in the different tests for the different conative attitudes, and especially to the different degrees of conation used. The rate and quality scores in all performances were recorded and these figures supplied suggestive but not conclusive corroboration of the introspections.

Space will allow of only a brief summary of the results of these lengthy introspective reports, which were remarkably consistent for all the subjects, and which were invaluable in aiding analysis of the quantitative results. These introspective experiments corroborated the original division of the tests into three groups, according as the minimal degree of conation essential for performing them was little (Tests 1, 4, 7), moderate (Tests 2, 5, 8), or great (Tests 3, 6, 9). With very great conation the speed instructions tended to cause emotional disturbances and confusion in all except the very simple Tests 1 and 4. The conation required was found to increase with the complexity of the mental operations, as will be more fully shown below.

VI. ANALYSIS OF THE QUANTITATIVE RESULTS IN THE LIGHT OF THE INTROSPECTIVE EXPERIMENTS.

A. *The Subjects and the Test Material.*

The results for the colleges, A, B, C and D, were from 56 subjects for the Tr and Sr experiments—and from 62 subjects for the Tq and Sq experiments. Thus a grand total of 2124 records of the Rate and Quality performances was obtained for quantitative analysis from students of an average age of 20 years.

In the schools, E, F, G and H, a grand total of 3852 records for analysis was obtained from 107 boys of an average age of $11\frac{1}{2}$ years, viz. from 30 boys in E, 31 boys in F, 27 boys in G, and 19 boys in H.

A set of nine tests having been carefully constructed for the colleges, the remaining three sets were made by following it as a model as faithfully in form and difficulty as was consistent with changing the material; so also in the case of the sets of tests for the schools.

The performance in the college Quality experiments was marked only for quality, but every other performance in colleges and schools was marked:

(i) for rate, by scoring the total attempted in the time allowed;
(ii) for quality, by means of the formula

$$q = 100 \times \frac{\text{Score right (R)} - \text{Score wrong}}{\text{Total attempted (T)}} = 100 \times \frac{\mathrm{R} - (\mathrm{T} - \mathrm{R})}{\mathrm{T}} .$$

The 'Score wrong' comprises errors and omissions.

The cyclic order used made it possible to compare the four sets of college tests, by collecting the Average-Scores of the experiments in which set 'a' was used, the Average-Scores for set 'b,' and similarly for sets 'c' and 'd.' It was found that there was no appreciable difference between the four sets of tests used for the colleges; similarly the equivalence of the four sets used in the schools was established.

In a similar way the groups were compared, viz. the four colleges and the four schools respectively; such differences as appeared were not large, but even if significant they were eliminated in the Attitude-Means because of the cyclic order used. By the same method the practice effects were found to be generally of the order and in the direction expected. All these checks are indications of reliability of the experimental results.

B. The Significance of the Results.

Because of the cyclic order used, the differences existing between the Attitude-Means must be due to differences in conation, other factors being the same in all the Attitude-Means (Table II). Now for every test, each Attitude-Mean was obtained by finding the Mean of the Average-Scores-in-that-test of the four groups of subjects. Each Attitude-Mean of Tables III and IV was thus obtained from four Average-Scores, each of which being the Average-Score of a different group of subjects had a different standard deviation. If, then, we are to discuss the differences between the Attitude-Means, it is essential that the significance of these differences be statistically determined. The omission of this tedious but necessary process has rendered much valuable work by other researchers well-nigh useless.

The great bulk of the differences in performance due to simple sampling lies within a range of three times the standard deviation, s (Yule (26)). Hence if a difference between two Attitude-Means, a and b, is to be attributed to a change in conation, that difference $(a - b)$ must be at least three times the standard deviation of that difference, *i.e.* $3s_{a-b}$. In finding this s_{a-b}, cognizance must be taken of the fact that a correlation exists between the performance of the same individuals in the same test under different attitudes. From Spearman's formula (22) for the s of sums and differences involving correlations, a new formula was derived to meet the special problems of this investigation (see Section VIII, below).

An examination of this formula will indicate the extremely arduous nature of the work involved in determining the significance of the results. Thus it was necessary to find 864 group averages, then 504 standard deviations, and 792 correlation coefficients, and these had finally to be used in the formula to find s_{a-b}. As a result of some months' calculation, the difference between the Attitude-Means could be expressed in terms of the standard-deviation-of-that-difference (s_{a-b}), as given in Tables V and VI. Differences less than $3s_{a-b}$ are insignificant and are recorded as zero. Irregularities in Tables III and IV were found to disappear as due to sampling effects, and the results could be readily analysed from Tables V and VI and definite conclusions could be arrived at.

C. Detailed Analysis of the Results.

The limits of space and the consequent need for brevity constitute a serious obstacle to clearness and completeness of exposition especially

ATTITUDE MEANS.

TABLE III. *College-Means.*

	Test 1		Test 4		Test 7		Test 5		Test 8		Test 2		Test 9		Test 6		Test 3	
	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q
Sr	135·41	97·64	30·30	94·81	49·58	98·01	56·83	89·29	16·44	86 93	40·62	96·89	80·86	77·03	21·56	81·12	113 36	79·60
Tr	176·30	96·91	33·91	92·97	62·76	97·66	73·98	87 85	22·65	86·49	50·79	92·96	92·61	82·04	27·98	73·33	124·13	80·41
Sq	—	95·28	—	92·68	—	96·96	—	90·18	—	89·21	—	95·74	—	82·62	—	84·37	—	91·99
Tq	—	96·88	—	95·90	—	98·81	—	96·52	—	93·79	—	97·81	—	89·79	—	90·64	—	95·14

TABLE IV. *School-Means.*

	Test 1		Test 4		Test 7		Test 5		Test 8		Test 2		Test 9		Test 6		Test 3	
	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q
N	98·78	90 55	16 34	63·21	27·55	95·76	36·70	75·63	13·35	35 88	37·25	60·37	81·49	44·84	33·53	7 60	100·81	40·16
S	86·20	92·53	15·17	66·27	29·06	96·26	31·28	79·15	13·11	42·91	38·53	56·52	80·41	49·11	31·59	16·36	107·81	36·83
Th	96·15	93·42	16·21	63·80	32·22	96·34	35·77	76·89	13·91	32·77	38·22	56·89	82 45	42 75	33·10	13·43	113 38	38·22
Ti	97·29	94·33	15·52	64·89	31·50	94·98	34·98	76·28	13·74	35·55	37·64	59 34	74·62	54·61	32·54	9 63	105 47	39·58

Each of the numbers in these tables is the Mean of the Average-Scores of the four schools (or the four colleges).

ATTITUDE-MEAN DIFFERENCES.

TABLE V. *Colleges.*

	Simple cancellation. Test 1		Facts sentence completion. Test 4		Number-checking. Test 7		Multiplication. Test 5		Sentence completion. Test 8		Same or opposites. Test 2		Complex cancellation. Test 9		Inferences. Test 6		Instructions. Test 3	
	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q
Tr-Sr	10·7	0	13 8	0	7·6	0	8·0	0	9·6	0	9 3	−3·3	4·8	0	6 0	0	2·9	0
Tq-Sq	—	0	—	0	—	0	—	3·9	—	0	—	0	0	4·3	—	0	—	0

TABLE VI. *Schools.*

	Simple cancellation. Test 1		Simple sentence completion. Test 4		Number-checking. Test 7		Multiplication. Test 5		Sentence completion. Test 8		Same or opposites. Test 2		Complex cancellation. Test 9		Inferences. Test 6		Instructions. Test 3	
	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q	r	q
Th-S	5·6	0	3·2	0	4·6	0	6 4	0	2·9	−3·1	0	0	0	0	0	0	0	0
Ti-Th	0	0	0	0	0	0	0	0	0	0	0	0	−3 3	4·2	0	0	0	0
Th-N	0	0	0	0	5·2	0	0	0	0	0	0	0	0	0	0	0	4·7	0
N-S	5 8	0	0	0	0	0	4 1	0	0	0	0	0	0	0	3·2	0	0	0

In the Tables V and VI, the differences between the Means given in Tables III and IV are expressed in terms of the standard deviation of the difference-between-the-Means-for-two-Attitudes. Where the difference is less than 2·8 times its standard deviation it has been given as zero.

in this section of the paper. This must be borne in mind if the analysis appears in any way dogmatic or insufficiently supported by arguments.

The results for each test will be examined separately, defining as briefly as possible the chief psychological processes involved in performing the test according to Spearman's *Principles* (19, 25), and so in the light of the introspective findings analysing the quantitative results for the colleges and schools.

Test 1—Simple Cancellation.

This test consists chiefly in apprehending the percept of the letter to be crossed out, and educing the relation of identity between the percept and the idea, reproduced or retained in consciousness, of the-letter-to-be-crossed-out. The test is so simple that the apprehension, retention, and eduction, both for adults and children, are well-nigh mechanical, and it requires very little conation for successful performance. A large increase in conation is thus possible, and this produces a corresponding increase in output by eliminating all unnecessary parts of the cognitive processes; *e.g.* by carrying the apprehension of irrelevant letters to a lower degree of clearness, by passing more rapidly from one letter or group of letters to the next and by speeding up the motor part of the performance. The test reduces in practice to a case of simple apprehension and motor-reaction; in such a simple operation the quality of the work is naturally high, so that the increased conation will tend to produce an improvement in rate rather than quality.

The experiments show that in the colleges increased conation produced no change in quality in the Quality experiments but a large change in rate in the Rate experiments (Table V). The schools show no significant change between the N, and Th and Ti experiments. This test, as the first of the set, tended to be done with great conation, and this with the novelty of the work resulted in the N Attitude approximating to the Th Attitude. Only when specially directed to adopt the S Attitude, did the boys attain a significant change in conation, as shown by the difference between the N and S experiments.

It is clearly shown, then, that in mental processes depending chiefly on simple apprehension, a large variation in conation while performing such operations is possible and produces a large corresponding variation in rate without an appreciable change in quality.

Test 4—Facts: Sentence Completion.

The introspection showed clearly that, though the test involves the easy eduction of the purport of the sentence and the identification of the reproduced item with the percept of the printed word among the four given, yet the performance of the test depends chiefly on reproduction. The facts used were so simple and familiar that their reproducibility was high; therefore very little conation was required to cause their re-occurrence. The test can therefore be done easily with little conation; a large increase in conation is possible; the correspondingly large increase in output is due to a more rapid cognition of the sentence, a speedier identification of the correct word, and so on. This is brought about by the elimination of all unnecessary irrelevant mental processes (such as full and clear cognition of all the four words given) through conative direction of the mental energy to the task in hand, and possibly also by increasing the absolute amount of mental energy (see (25)).

As expected then in such a simple operation, the Quality experiments in the colleges show no change, but the Rate experiments produced a large change (Table V). In the schools, as the tests were more difficult and more interesting to the children than to the adults, the S tended to approach the N Attitude, and the N Attitude the Th Attitude; the difference between the Th and S Attitude results, however, corroborate the college experiments.

Thus in simple mental processes depending largely on reproduction, a large variation of conation while doing these operations is possible in proportion to the reproducibility of the items; and in so far as variation in conation takes place there is a corresponding variation in rate without any great change in quality.

Test 7. Number-checking.

This test affords the simplest example of Spearman's second principle of cognition; it involves the eduction of relations on the perceptual level. The numbers are often compared as visual percepts, or chiefly as motor-auditory percepts, as shown in the introspections. Here there is apprehension of items and the cognition of the relation between the items. Too little conation results in too little intensity of cognition, and then the items are not apprehended clearly enough to be related, so that the process fails. Again under speed conditions, the items may be apprehended so rapidly in succession that the cognition of the relation has not time to reach the necessary cognitive clearness—this accounts for the periods of 'mental blankness' reported in introspections of Test 7 and Test 2, which may also be partly due to too much energy going into trying (see (25)).

This simple test can be done easily with little conation, and more quickly with greater conation, through elimination of irrelevant mental processes. As expected, in the Quality experiments in the colleges, there is no change as the test is so simple, while in the Rate experiments increased conation produced increased rate (Table V). In the schools, Test 7 has not the novelty (due to position in the set of tests) of Test 1, and is not so interesting as Test 4; hence the N Attitude approximates to S here, and both N and S are lower than Th (Table VI).

The conclusion is thus reached that in the eduction of simple perceptual relations, there is possible a large variation of conation in which these operations can be performed, and the occurrence of such conative change produces a corresponding large change in rate without affecting the quality of the operations.

Test 5. *Multiplication.*

The introspections clearly indicated a new element in this test as compared with Tests 1, 4, and 7, namely, the greater conation necessary to retain several items in the cognitive field (carrying figures, etc.) simultaneously with the occurrence of other cognitive processes, which here chiefly consisted of reproduction of items from the multiplication tables. (The part played by the eduction of conjunctive relations in adding is small compared with the importance of the reproduction processes.) Thus a moderate degree of conation is essential to do the test at all satisfactorily, but with the possible increased conation there is clearer apprehension, speedier reproduction, better retention, and avoidance of repetitions, so that there is an increase in rate.

The college Rate experiments show increased rate without change in quality, but in the Quality experiments there is a gain in quality because there the unlimited time allowed repetitions to check the results of the reproductive processes (which are liable to errors)—Table V. In the schools, the N Attitude approximates to the Th, which is natural in a school subject under test conditions. The drop from N to S would be expected to occur as the result of the definite instructions for S (Table VI). H. T. Moore (12), Moore and Gilliland (13), Courtis (4), and Thorndike ((21), p. 136), Geissler (7), Knight and Remmers (9), Chapman and Feder (3) have performed experiments which indicate the effects of conation on simple arithmetical processes, and corroborate these results.

The conclusion indicated is that in complex reproductive processes, conation is required in proportion to the extensity of the simultaneous cognitive operations (retention in consciousness and reproduction); that the possible variation in conation is greater if the extensity of the operations involved is smaller; that increased conation produces (*a*) an improvement in quality with unlimited time, by repetition of the reproductive processes for checking purposes, and (*b*) an increased rate without change in quality under speed conditions, by the exclusion of all irrelevant cognitive processes to increase both speed of reproduction and extensity of cognition, and possibly also by increasing the absolute amount of mental energy.

Test 8. *Sentence Completion.*

In this test each sentence comprised as essentials a fundament and a relation (usually causal); the cognition of these results either in (*a*) an eduction of the correlate notion followed by a reproduction of the word, or (*b*) the direct reproduction of the word from past experience, or (*c*) the occurrence of both these processes simultaneously. If the fundaments and relations have been often cognized together before (as is the case with adults) the process depends more on reproduction than eduction. Thus the college results were found to approximate to those of Test 4, though the smaller difference between Sr and Tr here shows that it cannot be done with quite as little conation as can Test 4 (Table V). For the schools, the test depends more on eduction than reproduction, and the increased complexity of the processes for children makes it necessary for them to use great conation. Hence the N, S and Th Attitudes show no change; the only change is a decrease in quality for the Th as compared with the S Attitude, which was, however, balanced by an almost significant and equal increase in rate—the conative emphasis appears merely to have shifted from quality to rate in changing from S to Th.

Mental operations which consist in a combination of eductive and reproductive processes involving extensity of cognition, require conation in proportion (*a*) to the inertia of the reproductive processes; for when such mental operations cannot be performed by easy and direct reproduction, the eductive processes become essential preliminaries to the reproduction and hence to the completion of the performance; but for effective eduction, determinateness of cognition, which follows on intensity of cognition, is required, and this last is controlled by conation; and (*b*) to the extensity of the cognition necessary. When the operation is one of easy reproduction, a large change in conation is possible and produces a corresponding change in rate, by elimination of unnecessary cognitive processes as in Test 4.

Test 2. *Same or Opposites.*

In this test as in Test 7 there is eduction of isolated relations, but here the eduction is on the conceptual not on the perceptual level. The meanings of the word-symbols have first to be reproduced and then cognized, and then the relation between these meanings has to be cognized. If the meanings have been frequently cognized in relation to each other previously (as may happen with adults) the relation may appear as a reproduct, in which case the test requires less clear cognition, as there is no eduction, and hence less conation. Hence the test is done more easily by adults than by children. But a moderate amount of conation is always necessary, because the cognition of the meanings must be determinate for effective eduction and this requires at least moderate conation. 'Mental blanks' are found to occur with great conation in this test just as in Test 7, but here they are much more prominent.

The college results (Table V) show a fair gain in rate at a loss in quality, thus supporting this analysis, while the lack of change in all the experiments in the schools (Table VI) indicates that the very nature of the test was a stronger determinant of the degree of conation than the teacher's appeal or the boys' voluntary intention; thus the boys used great conation in spite of instructions to adopt the S Attitude.

The eduction of relations on the conceptual level then requires intense cognition to bring about the determinateness in meaning necessary for the relation educing, and hence requires considerable conation both for the speedy reproductive development of the word-meanings and for the clear (determinate) cognition of those meanings.

Test 9. *Complex Cancellation.*

This test depends first on the cognition of a considerable number of fundaments and relations in the form of a rule; thus while retaining this rule in consciousness it was necessary to apprehend pairs of letters, relate them to the complex rule (retained or reproduced) and hence either cancel or leave the pair of letters being cognized. The introspections showed that the simultaneous extensive and intensive cognition involved in successful performance of the complex processes of this test could not be maintained with little conation.

The complexity of the test ordinarily makes for many errors. With unlimited time checking is possible and hence in the Quality experiments of the colleges (Table V) increased conation improves the quality; the improvement in the rate of the Rate experiments is obtained by avoidance of these checking processes. The small change in results shows that the test cannot be done with little conation. In the schools, the test was so difficult that the boys always used great conation, whence the N, S and Th Attitudes all show no change. From Th to Ti there is a change in the direction of the conation, the loss in speed being balanced by a gain in quality.

It becomes apparent then that in complex operations, involving reproduction, eduction and extense and intense cognitive processes, the amount of conation tends necessarily to be great; an increase in conation, if it can occur, tends either (*a*) to increase the quality, by repetition to check the result; or (*b*) to increase the rate, largely by eliminating this repetition, but also by speeding up the relevant processes (through concentrating the mental energy and possibly also through increasing the absolute amount of mental energy).

Test 6. *Inferences.*

The test involves a sequence of relation and correlate eduction of a very complex nature, and practice helps one to arrive at the final correlate in the least number of eductive processes ((19), p. 99). The test was found to be unreliable for schools, their results being the product of guessing instead of eduction. The necessity for extense and intense simultaneous cognition makes it possible for the higher order conjunctive and evidential relations involved to be educed only with great conation, hence a great change in conation is not to be expected. The improvement in rate in the colleges (Table V) is attained by concentration and hence more efficient eduction and by elimination of repetitions. No change in the quality occurs, even in the Quality experiments; the 'taking of risks' here affects the whole of a question, and hence being unpleasant, is avoided—this test by its nature impels accuracy. In Test 9 a risk only affected one item and was worth taking. This explains the difference in the results of these two tests.

For the (relatively) rapid eduction of higher order relations between complex fundaments much conation is required, for the extensity and intensity of the cognition involved are both great, and all available mental energy must be directed to this end and all extraneous mental processes must be eliminated. Only a small increase in conation is at all possible, the increased rate with no change in quality being attained chiefly by eliminating the repeating of cognitive processes for checking. With little conation the cognition fails in extensity or intensity, or both, and the higher order relations fail to be educed. As we shall see this same argument applies to Test 3 to be discussed next.

Test 3. *Instructions.*

For a number of reasons the school results were found unreliable in this test as in Test 6. The college results, however, can be relied on as showing a not quite significant change in rate with no change in quality. The same general arguments apply here as to Tests 6 and 9. The test requires intensity and extensity of cognition to be done at all; the conation necessary to maintain these is high and hence no change in conation is possible from S to T; in other words the conation is always high if the test is to be done at all. The checking processes are not sufficiently reducible to allow of a significant change of rate, so that in all the experiments the conation was probably the same and the amount of cognitive processes unaltered.

VII. SUMMARY AND CONCLUSIONS.

The findings of the research which has been briefly outlined above, together with the findings of the previous article by the present writer (25) will here be summarized as bearing out the general conclusions on which the experimental results are explicable.

A. Basis of the Investigation.

The basic theory on which this investigation rests is the Hormic Theory of Action as re-stated by McDougall. On this theory all human activity springs ultimately from our instincts; "by the conative... force of some instinct every train of thought...is borne along to its end." All mental activity is therefore purposive, it is a teleological process exhibiting the three aspects—cognitive, conative, and affective. "Our intellect or cognitive organization, is essentially the instrument of our purposes; it is brought into play in the service of our desires, the driving forces of the organism," which "are identical with the impulses, the conative tendencies of our instincts" ((11), p. 417). Accepting this fundamental theory of the relation "of conation, cognition, and affection," an analysis of the cognitive aspects of mental processes was found in Spearman's Principles of Cognition while the analysis of the conative aspects was based on Aveling's Principles of Conation (1). The emotional qualities of experience are regarded as indicators of the nature of our impulsive reactions, while affection serves merely to reinforce or divert our conative tendencies. 'Emotion' as having the impulsive force of an instinct is treated in the general problem of the influence of conation on cognition (25).

The fundamental principle through which conation influences cognition in the purposive functioning of our instincts is the quantitative anoegenetic Principle of Mental Energy:

"Every mind tends to keep its total simultaneous cognitive output constant in quantity however varying in quality. The total output must be regarded as the mathematical product of extensity and intensity. The amount and direction of the disposable energy regulate respectively the intensity and the quality of the ensuing mental process."

According to the instinct or sentiment in action we strive to reach certain ends; this conation results in a certain disposition of the mental energy in order to attain those ends. The amount of mental energy available tends, according to the Mental Energy Law, to be a constant. Whether the absolute amount of energy can vary with conation has still to be determined. This energy is used in the cognitive, conative and affective processes involved in attaining the desired ends and according to these ends these mental processes may vary in quality and amount.

B. The Quantitative Results.

An examination of the evidence advanced in this thesis provides ample support for these conclusions; in general the improvement attained decreases as the amount of the cognitive processes involved in the performance increases.

In the experiments on the apprehension of sentient experience, where the extensity of the necessary cognition is a minimum, it was found that a change in conation produced a corresponding change in the intensity and clearness of cognition of the sensation. Further the introduction of distractors results in an increase in the extensity (or amount) of the simultaneous cognitive processes (apprehension of the distractor as well as of the stimulus) so that when the conation is constantly great, the introduction of distractors caused a decrease in the intensity and clearness of the cognition of the stimulus sensation.

In processes of simple perception, such as crossing out letters in Test 1, where the amount of cognition, especially in respect of extensity, is small, a great improvement in performance arising from the greater possible increase in conation is obtained. Geissler (7) found that the performance in crossing out circles with great conation was adversely affected by such distractions as simultaneous adding, or calculating, or memorizing, and this falling off in performance was correlated with decrease in clearness of cognition, which is in accord with the conclusions stated above.

In Test 4 we have simple operations consisting largely in easy reproduction, and, the extensity of the cognition involved being small, a little conation only was necessary to concentrate the mental energy sufficiently for the reproduction of the requisite word by the Mental Energy Principle in accordance with the Principle of Retentivity.

In Test 7, the eduction of isolated concrete (perceptual) relations also involves a minimal extensity of cognition and only a small amount of conation is necessary. This allows of an improvement of the same order as in processes of isolated perception and isolated easy reproduction.

In these three tests we have fundamental processes in a very simple and unitary form, and the small extensity of the operations involved (apart from their intensity) allows of their performance with little concentration of mental energy and hence little conation. As the mental operations to be performed become more complex, *i.e.* as they involve more numerous simultaneous apprehensions, eductions and reproductions, so it becomes necessary to eliminate all extraneous operations, and to concentrate all the available and attainable mental energy on these processes; greater conation must therefore be aroused if the end is to be attained. The analysis of the results clearly showed that this was the case. Thus Opposites required greater conation than Number-Checking, Multiplication and Sentence Completion with eduction than Facts, Complex Cancellation than Simple Cancellation, and Inferences and Instructions than any of these.

Extensity may involve simply the apprehension of several items simultaneously, or the retaining of an item in the field of consciousness, while other processes of apprehension, or eduction, or reproduction are proceeding; or the eduction of higher order relationships, together with retention in the field of cognition of apprehended fundaments and relations; and so on in increasing complexity.

C. *General Conclusions.*

(*a*) *Effects on the qualitative cognitive processes.* The evidence adduced in this research gives no indication that the conation varies with the quality of the cognitive process, *i.e.* it does not seem to require greater effort to apprehend experience than to educe relations or to educe correlates. Nor would the nature of the qualitative principles lead one to expect such differences. If there are any such effects, they are insignificant by comparison with the greater effects produced by conation on the *amount* of the cognitive operations directed to the attainment of specific goals.

(*b*) *Effects on the amount of the cognitive processes.* Assuming as a fact of experience, supported by the experimental evidence cited previously (25), that we do strive little or greatly, according to the instincts or sentiments at work in us at the moment and the degree to which they are aroused, the general conclusions to which the evidence leads may be formulated as follows:

(1) In the attainment of any instinctive end, conation tends to be aroused in respect of cognition in proportion to the amount of the simultaneous cognitive processes necessary to attain that end.

(2) The increased conation tends to produce the increase in the amount of the simultaneous cognitive processes necessary to attain that end—(by withdrawing mental energy from irrelevant mental processes and concentrating this mental energy for the relevant mental processes; and possibly also by increasing the absolute amount of available mental energy).

(3) Increased conation may produce the increase in the amount of relevant simultaneous cognitive processes either in intensity, or in extensity, or in both, until all the disposable or attainable energy is being used in cognition to the desired end. When the amount of the cognition for the attainment of the end has reached its limit, the intensity and extensity of the cognitive processes must vary inversely according to the Mental Energy Principle.

(4) When the amount of the necessary cognitive processes is small, the end may be attained with little conation, so that a large increase of conation towards the end is then possible and this produces a great improvement in performance. As the amount of the cognitive processes necessary for the attainment of the end increases, the conation necessary for the concentration of mental energy for those processes also increases; the possible variation in conation therefore decreases, and hence the possible improvement in performance decreases.

(5) In the qualitative process of apprehension, the evidence shows that conation increases the intensity of the cognition and apparently the rate of rise of such intensity: hence by increasing the clearness of cognition of the fundaments, conation increases the efficiency of the qualitative processes of eduction.

(6) In the quantitative process of reproduction, (*a*) conation exerts an influence on the *reproducibility* of an item, in that the conation determines the intensity and the fullness of its cognition in learning; that is, with greater conation in learning the new item is apprehended more intensely and in relation to a greater number of cognitive items,

especially those round which sentiments are formed and which therefore have the impulsive force of the instincts tending to reproduce them; and (*b*) conation exerts an influence on the *reproduction* of a previously cognized item, in that by concentrating the mental energy, it brings the reproductive processes more prominently into action through the Mental Energy Principle; the actual reproduct being determined by the Retentivity Principle.

(7) The results showed a very general tendency for the increased conation to produce an improvement in rate without change in quality. Even when the conation was directed especially to quality, no improvement in quality was effected, except in those complex mental operations (*e.g.* Multiplication and Complex Cancellation) where faulty reproduction was liable to introduce errors. Insistence on rate tended to result in a loss of quality in mental operations involving eduction on the conceptual level (*e.g.* Opposites); for when insufficient time is given for full development of the meanings, the eductive processes become inaccurate.

D. *Future Investigations.*

It has been clearly established, then, that in all cognitive operations conation exerts an influence, and its importance in performance increases with the increase in the amount of the cognitive processes involved. The importance of directing research to a more detailed study of this problem is apparent for such work as that of Bronner (26) which showed the influence of conation in performance in 'intelligence' tests. What is urgently needed is some means of measuring the conation an individual is directing to the performance in hand, so that the experimenter can discover easily and speedily whether an individual's 'intelligence score' really does represent his maximum ability. The attempts of H. T. Moore, Gilliland, Morgan, Wells and Dodge to measure conation (25) while interesting and suggestive, are not of much use for this purpose; for those which do not require complicated apparatus and individual testing do not promise much in the way of exactness of measurement. The present writer has not discovered any comprehensive attempts elsewhere to arrive at an objective measurement of conation.

Research might be directed then to obtaining some form of measurement which can be speedily given to a group of subjects in the same way. By devising some mental test or series of graded tests in accordance with the findings of this preliminary research, which shall involve great conation while being largely independent of 'g' (Spearman's General Factor), a relation between performance in the 'g' tests and performance in the conation or 'c' tests, when both are done with maximum effort, might be found. An individual doing both 'g' and 'c' tests at the same sitting would give performances which, by comparison with the established relation, should show whether his 'g' score represents his maximum ability or not. By working along these lines a series of tests finely graded for the amount of conation necessary for their performance might be devised, but as the work up to the present time has only brought us to the fringe of the problem, it is obvious that considerably more groundwork must be accomplished before the measurement of conation can be attained.

In general the effects of conation on cognition have been found to be largely positive, that is, the immediate end tends to be attained more efficiently with increased conation. When the end is more remote, as in complex cognitive processes, the conation may result in failure through insufficient emphasis on the intermediary processes in a desire for speedy attainment of the remote end. Great conation may sometimes have a negative influence, in that so much energy may be put into the trying that too little remains for the cognizing; but more evidence is required with regard to possible negative influences of conation. Experiments should be devised where the mental operations are performed in the service of a powerful instinct strongly aroused; *e.g.* a subject, striving greatly to flee, might be made to perform specific cognitive processes of varying degrees of complexity as a means of satisfying the impulses of flight. By some such means we may be able to determine how far conation in itself can ever exert a negative influence on cognition.

VIII. MATHEMATICAL FORMULAE USED.

Spearman (20) has given a formula for the standard deviation of a difference between a and b, where correlations exist between the performances a and b, viz.:

$$s_{a-b} = s_a{}^2 + s_b{}^2 - 2r_{ab} \cdot s_a \cdot s_b.$$

The problem in this research was complicated by the fact that the difference, whose standard deviation was required, was a difference between two Means, each of which was the Mean of four Averages. (See p. 340 above, and Table II.)

If 'a' is a performance in a test under one attitude;
'b' is a performance in the same test under another attitude;
$\bar{a}$, $\bar{b}$ = the average scores of a group under the respective attitudes;
$\bar{\bar{a}}$, $\bar{\bar{b}}$ = the Means of the Averages a, b for all the N groups for the two respective attitudes;

r_{ab} = the correlation between performances of the same individuals in the same test under the two attitudes;

N = the number of groups tested (in this research, $N = 4$);

n = the number of individuals in a group (n varied from one group to another);

Σ = the summation for the N groups;

then it can be shown that

$$s^2_{\bar{a}-\bar{b}} = \frac{1}{N^2}\left\{\Sigma\left(\frac{s_a^2}{n}\right) + \Sigma\left(\frac{s_b^2}{n}\right) - 2\Sigma\left(\frac{s_a . s_b}{n} . r_{ab}\right)\right\}.$$

In this equation

$$s_a^2 = \frac{\Sigma(x^2)}{n}; \quad s_b^2 = \frac{\Sigma(y^2)}{n}; \quad r_{ab} = \frac{\Sigma(x . y)}{n . s_a . s_b},$$

where Σ is now the summation for all the individuals in a group;

x = deviations of individual scores in a group from the average score for that group under attitude 'a.'

y = deviations of individual scores in the same group from the average score for that group under attitude 'b.'

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(*Manuscript received* 18 *June* 1927.)

AN UNUSUAL CASE OF COLOUR-BLINDNESS

BY MARY COLLINS.

(*From the George Combe Psychological Laboratory.*)

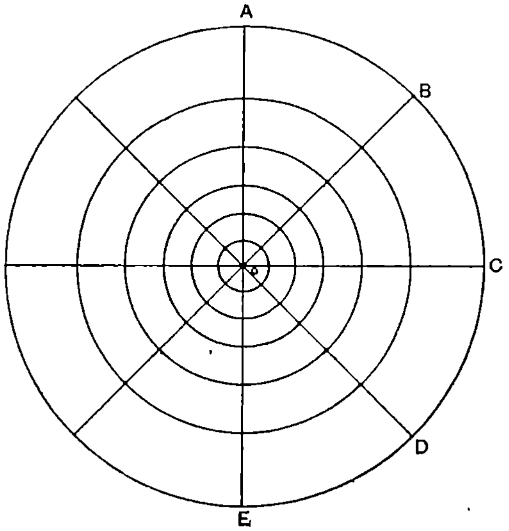
IN this case of colour-blindness, confusion seems to exist between blue and green, with apparently reduced sensitivity to other colours. With the Holmgren wools, S. matched a red skein with reds, but a green skein he matched with blues, and on being given a pale-blue skein, he chose greens as perfect matches. Stillings' Tables he could read, but not easily; he had to trace out the figures in the majority of the tables, and took much longer to read them than the normal visioned would do. The Ishihara tests for colour-blindness he deciphered in a similar manner. The Nagel card tests did not diagnose his defect as one of red-green blindness, nor as one of a serious nature. With the cards containing the reddish spots, he only omitted one of those which ought to have been included; the card containing 'reddish spots only' was correctly picked out; the card containing 'green only' gave a threefold response, the correct card and two others containing greyish spots, suggesting perhaps a slight weakness in the greens. In section B of the same test the colour naming was normal.

Tested with the Edridge-Green Spectrometer, he gave the following results (a gas-filled lamp of 60 watts was used as the source of light). His red began at 694 $\mu\mu$, showing a slight shortening. The yellow stopped at 571·5 $\mu\mu$, at which point blue and green ran on together until 471·5 $\mu\mu$. Then S. saw very faint blue or purple-grey until 443·5 $\mu\mu$, indicating a shortening at this end also. S. was completely unable to separate out the green from the blue and could draw no dividing line between them.

A second test with our own spectrometer in the George Combe Psychological Laboratory confirmed these results, a shortening at the red end as before, and yellow visible until 572 $\mu\mu$, at which point S. placed the transition from yellow to green. He continued to see green till about 455 $\mu\mu$ where a change in colour to blue took place. Beginning then at the violet end of the spectrum, S. saw violet up to 455 $\mu\mu$ and then a change of colour took place to blue which continued up to 572 $\mu\mu$, the transition point to yellow. That is S. called the range of spectrum from 572 $\mu\mu$ to 455 $\mu\mu$ (the blue and green regions of the spectrum), *green* when going in one direction, and when going the opposite way he called the same region *blue*.

This may appear clearer in schematic form

Normal		Red	Yellow	Green	Blue	Violet
1st time	(Red to Violet) →	Red	Yellow	Green		Blue
2nd time	(Violet to Red) ←	Red	Yellow	Blue		Violet



His field of vision was found to be very much restricted, particularly for blue and yellow. If we name the lines *A*, *B*, *C*, *D*, *E*, as in the diagram, the circles being 20°, 30°, 40°, etc., then his field of vision is for the right half of the right eye as shown in the table below.

	Red	Green	Blue	Yellow
A	34°	20°	15°	21°
B	40°	36°	10°	22°
C	35°	42°	10°	22°
D	35°	40°	10°	22°
E	25°	38°	10°	20°

The field for blue and yellow is considerably restricted and is less than the field for red and green, which is most unusual. It was, in fact, this peculiarity which first led to the investigation of his defect, for it seemed to point to a weakness for blues and yellows. From other tests, however, there seems to be no weakness of any kind in the yellows, nor confusion of yellow with any other colour. The colour anomaly seems to be restricted to a marked confusion of blue with green, a confusion of which S. has always been aware.

Colour mixing carried out with the Hering papers with the constant light of a daylight lamp of 60 watts gave marked proof of his defect. The following equations were obtained:

360° blue = 80° green + 280° black (very strong green and blue visible to E.)
36° red + 300° white = 336° white + 24° black (almost correct)
110° yellow + 250° white = 315° white + 45° black (cream to E)
104° green + 256° white = 301° white + 59° black (very pale green to E.)
160° blue + 200° white = 220° white + 140° black (pale blue to E.)
Blue = violet
Green = yellow } no match could be obtained
Blue = yellow
Blue + yellow = black + white (normal equation).

The first equation was very striking, a decidedly vivid green appeared to be identical to S., with an equally decided vivid blue. Further, it will be seen that the threshold for blue is high, for blue was quite distinctly seen by E. although the disc appeared grey to S. The threshold for green is also higher than normal.

The Rayleigh equation gave a big variation from normal, which suggests colour weakness in green.

	Red	+	green	=	yellow	+	black	+	white
Normal	153°	+	207°	=	52°	+	232°	+	76°
Subject	165°	+	195°	=	55°	+	227°	+	78°
	117°	+	243°	=	77°	+	202°	+	81°

The results of our tests seem to be that S.'s defect takes the form of green-blue confusion, combined with slight colour weakness in greens and blues.

Dr Edridge-Green has described such cases in his four-unit class.

(*Manuscript received* 12 *September* 1927.)

OBITUARY NOTICE

BY CHARLES S. MYERS.

HENRY RUTGERS MARSHALL.

1852–1927.

WITHIN the last few months Psychology has had to deplore the death in America of two of her most eminent and loyal workers—the death of Edward Bradford Titchener which occurred on August 3rd, 1927, a steadfast Englishman despite his long residence in the United States, and that of Henry Rutgers Marshall, who, though American-born, was typically British in his psychological outlook, repeatedly acknowledging in his writings his indebtedness to Croom Robertson, Sully, Ward and Stout. Of these two lamented psychologists, Marshall had accepted the invitation of the British Association to attend the proceedings of Section J in 1927 as its guest. Only in April last he wrote to me expressing his delight in, and his keen anticipation of, his coming visit. But in May, after a short illness, he passed away, having nearly attained his seventy-sixth year.

Henry Rutgers Marshall was born in New York in 1852. He graduated at Columbia University as Bachelor of Arts in 1873, and as Master of Arts in 1876. In 1878 he entered on his professional career as an architect. But his real abilities and aspirations could never be restricted to the mere planning and construction of beautiful buildings. Throughout his life, his fundamental interests were in the Theory of Art and in the Nature of the Beautiful. These led him first to the study of Psychological Aesthetics and thus to the study of Psychology in general. He continued to practice as an architect for many years, although his name will endure rather for his work in Aesthetics and Psychology. He was invited to lecture on Aesthetics at Columbia University from 1894–5, at Yale University from 1906–7, and at Princeton University from 1915–17. In 1907 he held the office of President of the American Psychological Association. He received the degrees of L.D.H. (Rutgers) in 1903, and of D.Sc. (Hobart) in 1910. Towards the close of his life, he acted as Executive Secretary to the Art Commission in New York, to which are referred all proposals for the erection of new monuments and other public works for consideration and approval from the aesthetic standpoint.

His married life lasted only seven years, and the tragic death not long after her marriage, of his only child, a lovely girl, was a further source of unconsolable grief to him. Resident in a New York Club, he suffered the lonely life of a widower, outwardly cheerful and sociable, although he admitted that he was surrounded by many friends and was occupied in interesting work.

In 1894 he published his first and best known book—*Pain, Pleasure and Aesthetics*. It was the outcome of several years' labour. Contemplation of works of art had impressed him with the important part played by Pleasure in our experience of the Beautiful. He was thus led to a careful examination into the psychology of Pleasure and Pain—'Algedonics,' as he termed the subject. At that time (35 years ago) there was frequent confusion among psychologists between Pleasure and Pain with Sensation on the one hand and with Emotion on the other. Marshall's sane and enduring conclusion was that—"Pleasure-pain...is not sensation, yet is closely bound up with sensation; it is not emotion, but is closely bound up with emotion also." It is interesting to observe that at this remote date Marshall clearly anticipated later writers in regarding emotions as special types of instinct experiences, due to what he terms complex 'instinct feelings.' Instinct-feelings, as he once wrote me, "correspond with Instinct Actions which look to the advantage of the organism...; whilst Pleasure-Pain relate to the advantage of elements and only in some cases, but indirectly, come to have to do with the welfare of the organism as a whole." With his usual acumen, Marshall distinguished two classes of pleasures—those induced by active functioning, and those connected with the cessation of activities. He likewise distinguished two classes of pains—those connected with excess of function, and those connected with failure to function. Finally he distinguished definite states, neither of pleasure nor of pain—states of indifference. He points out that the pleasures and pains of action are local, whereas the pleasures of rest and the pains of restriction must be general or systemic. He suggests that aesthetic pleasures differ from non-aesthetic pleasures in that the former are relatively permanent in revival, whereas the latter are not necessarily so. It is, he believes, due to this character of aesthetic pleasures that we experience beauty, and to the same character of aesthetic pains that we experience ugliness.

This first book of Marshall's rapidly became and still remains a classic. But he always complained that his accompanying physiological theory of Pleasure and Pain which he, like myself, considered relatively unimportant, attracted nearly all the attention that the book received.

I have therefore restricted myself to indicate its psychological importance. In a review of the book William James praised him thus—"His own learning is admirably complete....The modesty of his tone is also remarkable....The book is full of shrewd and original psychology. All these qualities render it almost 'epoch-making' in the present situation of science." James's words admirably describe the character of the author; he was "learned," "modest," "shrewd" and "original"; but above all, he was a man of great breadth of mind and of unswerving honesty, thorough in method, cheerful in disposition, and ever ready with kindness and sympathy.

In the following year, 1895, Marshall published his *Aesthetic Principles*, and in 1924 he issued a more popular work on the subject which he had completed before the War, entitled *The Beautiful*. Between these years, he published—in 1898 *Instinct and Reason*, in 1915 *War and the Ideal of Peace*, and in 1919 *Mind and Conduct*.

But these were minor works in comparison with the large volume of nearly 700 pages on *Consciousness* which he brought out in 1909. Part of this work, he tells us, he had already with his usual thoroughness prepared in 1891 when he was writing his *Pleasure, Pain and Aesthetics*. His desire then was, he says, to "bring all related psychological facts into harmony with" his theory. This part, elaborated in greater detail during later years, forms the second book of his large work on *Consciousness*: to it he added Book I devoted to "Consciousness in General" and Book III which he called "The Self."

In this somewhat neglected book, one of Marshall's chief aims was to indicate the light that considerations based on psycho-physical correspondence may throw on the strictly psychological aspect of the mind, to set forth his belief that the Self is fundamentally of the same nature as the presentations made to the Self, that the Self is merely the complex, unemphasized, undifferentiated mass to which by noetic emphasis presentations become presented as such, and that the Self, when presentatively given, appears as the Empirical Ego.

Marshall's interest in Psychology did not lessen with advancing years. His death robs us of one of that small but important group of persons who, without holding academic position and with other occupations, have made valuable contributions to the subject.

PUBLICATIONS RECENTLY RECEIVED

The Psychology of Religious Mysticism. By James H. Leuba. London: 1926. Kegan Paul, Trench, Trubner & Co., Ltd. Pp. xii + 336. 15*s.* net.

In spite of his strong convictions about religious problems in general Prof. Leuba has attempted to discuss the most difficult and problematic of them all in a scientific manner. It is one of the claims of the scientist to give an impartial record of facts and an unprejudiced judgment of such explanatory notions as he may indulge in. The readers of this book must form their own opinions, but for my part I think the author has exercised admirable restraint in the face of the most provoking situations. He has attempted to study the *facts* about mysticism and to draw unbiased conclusions. That is to say he has attempted to draw his conclusions from these facts and not from the innumerable ideas and attitudes which have been maintained before his time. For a mystic there is nothing controversial about mysticism. The intellectualist sceptic may offer a heartfelt prayer when the stress of circumstances demands it. To whom or what will he pray? When confronted by his intellectual companions he will be driven to deny the existence of any thing or being to which he could possibly attempt so logically impossible a relation as that of supplicator, in this universe of reasons and causes. But when the occasion for prayer comes the event is absolutely true of itself and takes precedence of any argument or proof. This is sheer mysticism, especially when a man is uplifted by such an experience. For a mystic there is no problem. His experience is true, real, and indestructible. He cannot doubt it; for it is the profoundest thing he does or that happens to him.

For a philosopher, on the other hand, there is a very big problem. If a mystical experience claims transcendent and ultimate reality then what is the status in the order of things of events which do not make these claims of us; and why should so few of us enjoy this enlightenment; or why should those who do be rendered almost insane in the process? Further what is the status of objectively similar events, or states, which seem to differ from mystical trances only in that their subjects do not claim profound enlightenment, or moral guidance, through them? But the biggest problem to the philosopher is to know what is the nature of the reality that the mystic claims knowledge of and by what means he attains contact with this reality. If the mystic is right when he finds that reality is not a thing with which he can enter into a relation, but that it simply is one and the same thing with his experience, then where does the realism of everyday life stand? The only reality which has any existential status is not distinguishable from one's own thoughts. To be a mystic seems at the least to involve the claim that life as the man-in-the-street knows it is just a bubble—bright and attractive while it lasts, but leaving nothing when it has gone. Hence the ultimate desire of the mystic is to make himself unconscious. When he has been unconscious he has really lived.

To the scientist mysticism also offers problems. What is the peculiar and extraordinary impulse which drives people to ruin their lives—as the scientist understands life—for the sake of a delusion that they are divinely inspired to help the world and themselves? If we all became mystics the world of science, that useful world of facts and information, would vanish. But putting this aside for a moment, are there not enough dements, and drunkards, and aesthetic wrecks, in the world already for us to be able to do without people whose religion—their moral guide—demands that they shall deliberately make themselves insane by drugs or protracted contemplation? So thinks the scientist, and he becomes interested in finding out the methods and impulses by which the mystic attains his goal. He finds, for instance, that mystics are for the most part emotionally injured. Often enough they are unhappily married. He feels that if he knew a little more about them he could cure them once for all.

These are some of the principal aspects of mysticism. Whether Prof. Leuba has supported his position in this book that by an adequate treatment of the scientific aspect of the subject he can show the other points of view to be of little value I must leave to the opinion of his readers. In his introductory chapter he distinguishes between the 'objective' and the 'mystical' types of religion. He then proceeds in the two following chapters to explain how mysticism arose from the use of poisonous drugs by 'uncivilized' peoples, and how, as civilization grew it became more customary to produce ecstasy by prolonged concentration and self denial, as in the *Yoga* systems. Chapter IV is a brief account of the origin and development of Christian Mysticism, together with a biographical study of five of the most celebrated Christian mystics: Henrich Suzo, Catherine of Genoa, Madame Guyon, Santa Thérèsa and Marguerite Marie. In a chapter on the "Motivation of Christian Mysticism" the author concludes that "Self-affirmation and the need for Self-esteem," the "Dread of Isolation; and Needs for Moral Support, etc.," the "Universalization or Socialization of the Individual Will," and the "Sex-Impulse" are the principal motives. He lays far more stress on the last of these than on any of the others. He finds that Christian Mysticism is almost always the outcome of a mal-adaptation of sex impulses and activities. In the next chapter the "Methods" of Christian Mysticism are discussed somewhat unsatisfactorily. The chapter tends to be rather an account of the stages of Mystical Experiences than a discussion of the methods used in bringing them about. The chapter on the relation of the moral development of mystics to fluctuations, or rhythms, in their psycho-physiological activity was worthy of a more elaborate treatment, for it is on a study of the levels of physiological activity, and fluctuations in these, that the real connecting link between psychology and physiology will be found. Prof. Leuba is perhaps a trifle too resistant to Freud, and a little too inclined to take his psycho-pathological information from Janet, albeit Janet has made a contribution to psychology of more general human interest than Freud, and consequently of more value to a study of mysticism. It is a pity the biographical psychology of Christian Mystics cannot be studied on the closely personal and yet unprejudiced way in which Janet studies his case-histories. But of course this is impossible as their lives are only known on tradition. After this unoriginal chapter on Hysteria there follow two good chapters of some length descriptive of Ecstasy and Trance Consciousness, a very poor one on the "Sense of Divine Guidance," and one on "Religious Science and Philosophy." The concluding chapter on "The Disappearance of Belief," etc., I think is too dogmatically "scientific" to be very inspiring.

Prof. Leuba has certainly made a good contribution to contemporary psychological studies, and it is interestingly written and well thought out. To what extent it is a satisfactory study of Mysticism is a problem for the reader to solve.

R. W. PICKFORD.

Mental Life. By B. EDGELL. London: Methuen & Co., Ltd. 1926. Pp. xvi + 269. 7*s.* 6*d.* net.

We have delayed this notice in order to be able to test Prof. Edgell's text-book in the way in which all such books must ultimately be tested, that is to say by giving it to beginners to read and seeing whether it both sustains their interest and gives them a reasonable knowledge of the subject matter which it purports to teach. On both these points *Mental Life* comes well out of the test. In the present welter of psychological writing it is refreshing to come across a book which is attractive without being wild, and in touch with actual life without being sensational. On the whole the book is perhaps best suited to those who come to psychology having had some general literary training and with a genuine interest in literature. But its appeal is certainly not confined to people with this kind of training and this kind of interest. It is in fact as good a general introductory text to psychology as is to be found anywhere, and a lot better than most. Beginning with a sketch of the nature of psychological problems, it offers a brief, but good, outline of the functions of the central nervous system, and proceeds to an examination of the essentially active basis of conduct in two chapters on Native Endowment and Environment, and on Primitive Values. Then comes the exposition of cognitive processes in their developing order of complexity, from sensing to thinking. And at the end are discussed the higher affective life and problems of Character and Temperament. An appendix deals with Mind and Body, and there is a brief Bibliography. All through it is well written, well informed, judicious and interesting. To each chapter a number of problems for the student are appended. It is a good and successful book and deserves very wide circulation.

Helmholtz's Treatise on Physiological Optics. Translated from the Third German Edition. Edited by JAMES P. C. SOUTHALL. Vol. III. The Perceptions of Vision. Published by the Optical Society of America. 1925. Pp. x + 736. With 6 plates.

This volume brings to a conclusion the great enterprise in translation undertaken by the Optical Society of America. In undertaking this work and in carrying it through with most conspicuous success Prof. James P. C. Southall has laid all English speaking students of visual processes under a lasting debt of gratitude. The third volume is of course the most interesting of all—from a psychological point of view. Every reader of Helmholtz, as he has studied the treatment of Perceptions of Vision, must have realized again and again the truth of the statement made in the editorial note to the present edition: "When one considers the comparatively limited means by which the sense of sight enables us to form more or less accurate conclusions about the nature and configuration of the various objects that are exposed to view in the visual field, and the amount of information that is conveyed to us in this way, the clearness and precision of the so-called Perceptions of Vision, which is the subject treated in this third volume, is indeed little short of marvellous." Dr Southall and his collaborators have surmounted triumphantly the many great difficulties of translation which this part of the work especially presented and have produced a version which is both eminently readable and accurate. There is, rightly and fortunately, not very much new material incorporated in the present edition. Two valuable notes by v. Kries are appended to the discussion on the perception of depth and on binocular rivalry. The first of these deals briefly with certain perceptions of depth which may arise from watching an object moving laterally or transversely in the field of view. When, under these conditions, the light coming to the one eye differs in intensity or colour from that coming to the other eye impressions of depth arise. Reference is also made to A. v. Szilly's pendulum experiment. The second note refers to Trendelenburg's experiments to demonstrate that under suitable conditions an actual process of binocular mixing of colours takes place.

To this volume are appended an exhaustive bibliography and a good index of subjects dealt with in all three volumes.

These three volumes of translation ought to be present and readily accessible in every psychological laboratory in this country. As the editors remark, and as the bibliography shows, a vast mass of work on the special sense of vision has been carried out from Helmholtz's day to the present time. But in all this British psychologists have played nothing like so important a part as they ought to have done. There are signs now of a reviving interest in the vitally important problems which the study of visual processes raise. There is great need for more research and for more instructed research. Towards the production of this these three volumes ought to contribute more powerfully than anything else that has appeared in English for a very long time. The work may be obtained by application to the Secretary of the Optical Society of America, Rockefeller Hall, Ithaca, N.Y., U.S.A.

An Approach to the Psychology of Religion. By J. CYRIL FLOWER, M.A., Ph.D. London: Kegan Paul, Trench, Trubner & Co. 1927. Pp. xi + 248.

Dr Flower identifies the religious reaction as one to situations which in some way baffle the mechanisms of the organism, by containing an element of 'beyondness.' If there are some situations which are fully 'controlled' by the organism in the sense that it can grasp the full chain of their events both backward and forward for a certain distance, then these situations do not elicit the religious response. There is nothing of 'beyond' in them. On Flower's position, then, some people can be said to keep their religious attitude for special occasions when they have a technique or ritual for producing it, which they call their Church. These people are often able to adopt an entirely cold-blooded attitude to the problems of every-day life, and often as not use religion simply as an escape from reality. Another type of people, who conventionally are non-religious, for they have no specific type of ritual, may, on Dr Flower's basis, be profoundly religious if they react to the 'beyondness' of the things they are interested in. The religious frame of mind then is not a phenomenon for psychopathology, in its own right, for it is a normal and healthy way of reacting to most situations. It is not even a peculiar type of mechanism which is brought into play at a certain level of complication; it can be a healthy part of almost any mental process. During the course of his argument the author is bound to discuss the phenomena of the religious response as if they occurred on very special occasions only, but he evidently intends to imply that they could occur at any time when there is a mysterious element in the situation. From this it is almost inevitable to conclude that there is a 'religious' temperament distinguishing some persons. This Flower regards as psychologically ultimate. He prefers to think of it as a plasticity of make-up which renders some more sensitive than others. In a sense it is his whole thesis that this sensitive temperamental balance should be substituted for a specifically religious drive or instinct, in psychological explanations. In his method of attacking his problem from the data of investigations by other workers who have special knowledge of their material Flower strikes upon an interesting method of work. This has been suggested by F. C. Bartlett and is really a distinct method of experimental psychology as distinguished from the old method of gaining facts from all and sundry experiences. The experimental psychologist must often be a clinician, making a close personal study of his subjects. When this is so it is not possible for the synthetic and theoretical worker who follows to have this personal touch in many cases, and he must take his information from the original worker. This is what Flower has done in his study of the Winnebago Indians.

Although it is only 'an approach' to the subject this is an illuminating volume; and is a distinct advance, psychologically at least, upon the other publications of this author.

R. W. P.

Mind and Body. By HANS DRIESCH. Translated from the German by THEODORE BESTERMAN. London: Methuen & Co., Ltd. 1927. Pp. xviii + 161. Price 6*s*.

This is a translation of the third German edition of the small volume in which Driesch puts forward again, and elaborates, some of the arguments and conclusions to be found in the *Science and Philosophy of the Organism*, and adds some new material. Probably the book suffers a little in the translation, though it has been done with great care. The English of this book is not up to the standard of Driesch's own English style.

Special Disabilities in Learning to Read and Write. By E. E. LORD, L. CARMICHAEL and W. F. DEARBORN. Harvard Monographs in Education, Series I, Vol. II, No. 1. Cambridge, Mass.: Graduate School of Education, Harvard University. 1925. Pp. 76.

Two cases of special disability are described. The first was one of word blindness as shown by inability to learn to read by the usual methods. By phonic analysis of words, and by practice in reading from a continuous context, the child was successfully taught to read, showing that his disability was merely functional, and probably the result of an unusual difficulty in associating the sights and sounds of words, and the kinaesthetic feelings of their articulation. The second case was one of mirror-writing. The child was of normal intelligence, but strongly left-handed. As he grew older, he ceased to use mirror-writing, and his normal hand-writing improved. The conclusion is that mirror-writing is largely dependant upon the natural initial difficulty of a left-handed child in learning to write from left to right, possibly associated with some emotional disturbance during the learning period. This monograph should prove useful to those specially interested in the subject.

Teaching Beginners to Read in England. By W. H. WINCH, M.A. Journal of Educational Research Monographs, No. 8. Bloomington, Illinois: Public School Publishing Co. 1925. Pp. 185.

This monograph gives a clear account of a careful research into the relative values of the phonic method (using ordinary print), the phonoscript (in which modifying signs are attached to the letters), the 'look-and-say,' and the alphabetic methods of teaching children to read. The children were first carefully divided into parallel groups by testing their ability to learn the use of certain arbitrary symbols; and one group was taught by each method. The phonoscript was found to be the best, and then the phonic; though both these systems failed to teach spelling satisfactorily. At first there was considerable loss when children, taught to read phonoscript, were required to read ordinary print; but this loss disappeared after about two years' training. The results obtained in this investigation appear to be well tested and thoroughly reliable.

The Psychology of Social Institutions. By CHARLES H. JUDD. New York: Macmillan & Co. 1927. Pp. ix + 346. 8*s.* 6*d.* net.

Prof. Judd shows in this book a number of important ways in which genuinely social factors determine individual thought and conduct. He deals with economic systems, with the psychology of number, punctuality, precision and exact thinking. He traces briefly the development of alphabets and the importance of language, which he treats as the social institution *par excellence.* Finally he discusses the social determination of aesthetic values, of religion, of science and of education. The book is full of interest and worthy of the attention of all students of social psychology. It is rather too wide in its scope to allow of much more than a surface treatment of the problems which it raises, but it will suggest to the careful student a mass of questions which he can pursue more intensively for himself.

Social Psychology Interpreted. By J. W. SPROWLS. London: Baillière, Tindall & Cox. 1927. Pp. xii + 268. 18*s.* net.

"This volume," says the author in his opening words, "is intended for students who are beginning the study of social psychology." They will find it rather difficult reading chiefly because Prof. Sprowls splashes enormous numbers of authorities all over his pages. Students of considerable maturity, however, could profit very greatly by a study of the views and arguments presented. In general Prof. Sprowls favours a historical approach to the subject but it is rather difficult to do complete justice to this within the limits of space which he has set himself. The last chapters in the book deal with Social Organization, Culture, The Conflict of Cultures, The Conflict of Groups, and Social Movement. All of these raise problems to the solution of which the social psychologist can contribute much without losing himself in speculation about the ultimate character of instincts, of the 'group mind,' or of 'social forces.' Both for what he summarises and for what he himself says on these problems Prof. Sprowls' book is worth reading.

The book has a really good bibliography and an adequate index.

An Introduction to Social Psychology. By L. L. BERNARD. London: George Allen & Unwin, Ltd. 1927. Pp. x+651. 18*s.* net.

Mr Bernard is already well known for an interesting study of Instinct considered in particular relation to Social Psychology. In this book he casts his net much further and endeavours to capture and study all the large variety of facts and theories that are relevant to the study of the social determination of conduct. After a general introduction he attempts a study of the human organism regarded as a complex biological organization. He regards conduct as the expression of numerous 'behaviour patterns,' and the social and mental sciences as mainly concerned with the manner of differentiation of these at levels of a high degree of complexity. The third part is more specifically germane to social psychology and attempts to show how that organization of behaviour patterns which constitutes personality can be directly affected by social factors. The fourth part, certainly not less interesting than the third, deals rather with group determining factors, the group being regarded as a whole or unit. There are bibliographies, rather too extensive, to each chapter.

The book is undoubtedly of great interest. But it is much too long. It would gain greatly by a condensation of style that could leave its matter practically untouched. A judicious reader may 'skip' a good deal, but he will learn much from Mr Bernard's discussions.

Psychological Healing. By PIERRE JANET. Translated by EDEN and CEDAR PAUL. London: George Allen & Unwin, Ltd. 2 vols. 42*s.* per set.

This book amounts to a critical history of Psychotherapy, and a profound, though cynical, study of human nature. Janet attempts to be essentially empirical in his method; he objects to all the methods of Psychotherapy which are rather theories of illness than curative processes. One should start out with the intention of helping people and not with that of erecting a speculative framework with the help of which to try to expound the causes of your patient's troubles. Hence it should be the method of the Psychotherapist to study all the various ways in which patients can be assisted, to systematize these into an organized set of methods, and to try to differentiate between illnesses so that the most successful method may be applied to each case. Then we should have a reputable system of Psychotherapy instead of a medley of theories, each the fad of some individual, none covering all the ground, but each used by its particular advocate as though it would apply to any case that came along. Thus he goes through all the methods which have had any success, tries to estimate that success, analyses the method with a view to pointing out what is good in it, and discarding what is bad, and limits himself to a rather obvious type of explanation in which the terms 'psychological energy,' and 'psychological tension' appear frequently, and of which perhaps the greatest advantage is its transparency. He is hardest on the method of Psycho-Analysis, for he regards this as being only a little less fantastic philosophy-and-water than Christian Science. But even then he has something good to say of it. Possibly the greatest fault of this book is its length. Granted that it is impossible to give a reasonable account of your patients and methods when you are a great physician, without a lot of space; but even then Janet is too discursive. It is well translated and admirably bound, and much less morbid reading than some other volumes on a similar topic.

Qu'est-ce que la Vie? By EUGENIO RIGNANO. Libraire Félix Alcan, Paris. Pp. 208. 20 francs.

Rignano passes in review all the manifestations of life which appear to be finalistic or purposive; starting with the physiology of assimilation and then considering regeneration, adaptation, behaviour, some mental processes, and morals, he proceeds to a critical account of mechanism and vitalism, and suggests a 'solution vitalistico-energetique.' He concludes with a chapter on man's place in the universe.

The Ego and the Id. By SIGM. FREUD, M.D., LL.D. Authorized translation by JOAN RIVIÈRE. London: The Hogarth Press. 1927. Pp. 88. 6*s*.

Freud's more philosophical writings are always of interest, and this small volume—No. 12 of the International Psycho-analytical Library—is no exception. In it the connections between the ego and the id, and the relations of both with the ego-ideal, are worked out. The book also helps to clarify the psychoanalytic conceptions of the origin of feelings of guilt, and of the three kinds of anxiety which develop as a consequence of conflicts between the ego and its three masters: the external world, the ego-ideal, and the id.

Idiosyncrasies. By HUMPHRY ROLLESTON. London: Kegan Paul. 1927. Pp. 119. 2*s*. 6*d*. net.

Rheumatic Diseases. By M. B. RAY. London: Kegan Paul. 1927. Pp. 91. 2*s*. 6*d*. net.

Diagnosis and Spiritual Healing. By F. G. CROOKSHANK. London: Kegan Paul. 1926. Pp. 101. 2*s*. 6*d*. net.

Dermatological Neuroses. By W. N. O'DONOVAN. London: Kegan Paul. 1926. Pp. 99. 2*s*. 6*d*. net.

These four brief monographs all belong to the Medical Series of the well-known Psychic Miniatures. Each in its own way belongs to the realm of the specialist, but they all have considerable general psychological interest for they all demonstrate very clearly how often the study of specific diseases can be carried out fruitfully only in the light of a consideration of the general reaction of the individual necessary to adapt him to his environment. Sir Humphry Rolleston has written an extraordinarily interesting account of idiosyncrasies mostly of an organic or physiological nature. It is to be hoped that somebody may soon do the same sort of thing for psychological idiosyncrasies. Dr O'Donovan shows clearly the necessity for clinical and psychological study in the investigation and treatment of affections of the skin, gathering into his small book a striking collection of cases. Dr Crookshank writes somewhat philosophically about the history and *rationale* of the theory of diagnosis, and adds an interesting paper on 'spiritual healing' which contends that in general only functional cases are successfully dealt with by this method. Dr Ray gives a clear and illuminating classification and description of the different kinds of rheumatic disease. The bulk of his treatment cannot be called psychological, but in his conclusion he maintains that psychic states, acting through the endocrine system, have an important bearing on metabolism and so are concerned in the production of various forms of rheumatism.

Possibility. By SCOTT BUCHANAN. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1927. International Library of Psychology, Philosophy and Scientific Method. Pp. 198. 10*s.* 6*d.* net.

This is an essay in philosophy, remarkably well written and attractive in that respect, but far from easy to follow. Various sorts of possibility, scientific, imaginative and 'absolute,' are distinguished. These are considered in relation to their ultimate formal constituents, called by the author 'parameters.' What is the final conclusion, except that philosophers must go on philosophizing, it is not easy to see, but the author in the course of arriving at his conclusion makes many attractive and challenging statements which produce a book that many will find well worth reading.

Inside Experience. By JOSEPH K. HART. With an Introduction by JOHN DEWEY. London: Longmans, Green & Co., Ltd. 1927. Pp. xxvi + 287. 10*s.* 6*d.* net.

It is not easy to characterize this book briefly. It makes out philosophy to be an art, treats experience as a part of nature, and nature as interpenetrated by experience, and is against all types of dogmatic and mechanical explanation. It belongs on the whole to the type of philosophic meditations which are supposed to provide 'uplift' and 'stimulus' more than anything else. The large number of people who enjoy this kind of thing will probably enjoy this instance of it very much.

Proceedings of the Sixth International Congress of Philosophy, 1926. London: Longmans, Green & Co. 1927. Pp. lxxxvii + 716. 25*s.* net.

This handsomely produced volume contains the papers read at the Philosophical Conference held last year at Harvard University. There is naturally extremely little of direct psychological interest, though reports are included of a number of discussions on epistemological topics which at least border on psychological matters. Accounts are also given of the business and social proceedings and a full list of members is included.

Superpersonalism: The outer consciousness. By W. D. LIGHTHALL. Montreal: The Witness Press. 1926. Pp. 105. 4*s.*

The view is maintained that consciousness is 'centred' outside the individual and numerous sweeping conclusions deduced from this view.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

November 2, 1927.	Some Individual Differences in Susceptibility to Accidents, by Dr W. V. Bingham (Director, Personal Research Federation, New York).
December 10, 1927.	Self Psychology, by Prof. Mary W. Calkins (Professor of Psychology, Wellesley College, Mass.).

SECTIONAL MEETINGS.

Education Section.

October 10, 1927.	Disability in Reading, by Miss E. G. Hume, M.A. Ed. (Lecturer, Graystoke Place Training College, London).
November 14, 1927.	Emotional States and their Relation to Eye Symptoms and Diseases, by W. S. Inman, M.B. (Senior Ophthalmic Surgeon, Portsmouth and South Hants Eye and Ear Hospital).
December 5, 1927.	An Experimental Study of Temperament, by D. W. Oates, M.A., F.C.P. (Municipal Secondary School for Boys, Newport, Mon.).

Medical Section.

October 26, 1927.	A Case of Claustrophobia, by Dr Emanuel Miller.
November 23, 1927.	Psychological Aspects of Birth Control, by Dr M. D. Eder.
December 19, 1927.	The Reliability of Infantile Memories, by Dr J. A. Hadfield.

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VOLUME XVIII APRIL, 1928 PART 4

THE UNCONSCIOUS IN RELATION TO ANTHROPOLOGY[1]

BY C. G. SELIGMAN.

IN this paper it is proposed to examine how far certain recent ideas dealing with the psychology of the unconscious can aid in the solution of some of the outstanding problems of anthropology. Before dealing with this question it is necessary to refer briefly to two matters:

(1) The first of these, the only result of the comparative examination of the senses of ourselves and of native races that need detain us, is the relatively high threshold for pain of at least some of the latter[2], a fact which must be borne in mind when considering the readiness with which many non-Europeans submit themselves to such mutilations as those discussed later in this paper[3].

(2) The second is that Lévy Bruhl's hypothesis that the savage thinks in a manner different from ourselves, that his thought is 'mystical' and 'prelogical' to such an extent that a new terminology is required to discuss his mental processes, has not been accepted so far as I am aware by anthropologists in this country, and is in my opinion in contradiction to the experience of field workers, who are after all the best qualified to judge.

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[1] This paper was read in outline at the Annual Meeting of the British Psychological Society in 1924, but owing to ill-health it has only recently been possible to prepare it for publication. Although the paper as now published does not differ in argument from the spoken version it has been expanded, and is, I believe, the clearer for the criticism that followed, so that I feel most grateful to those who took part in the discussion. I also desire to acknowledge the constant assistance rendered by my wife and to thank Dr Roheim for reading the paper and making a number of suggestions.

[2] The Murray Island men examined by McDougall had a susceptibility to pain hardly half as great as Englishmen (*Reports of the Cambridge Anthropological Expedition to Torres Straits*, vol. II, p. 195). My own experiments on jungle Sinhalese gave a rather lower threshold for pain, but they were still well above the English threshold, 5·5 as against 3·8 (*The Veddas*, p. 407).

[3] It is assumed that this insensitiveness extends to the more extreme degrees of pain, an assumption supported by the lack of shock after severe injuries, constantly observed by travellers among the more primitive peoples. For an extreme instance see Roscoe's account of the circumcision ceremonies of the Bagesu (*The Bagesu*, Cambridge, 1924, pp. 31 and 32).

I would also ask you to believe that no value need be attached to hypothetical reconstructions of what a savage might do under certain impossible experimental conditions and I hold it to be true to say that there is no basic difference in mode of thought of savages and of ourselves, but only quantitative differences, and that apparent dissimiliarities are to be explained by a different *weltanschauung* best summarized by the statement that the categories of non-European races, both savage and civilized (*e.g.* the Chinese), differ from our own.

Having to some extent cleared the ground I may indicate the headings under which it is proposed to examine and discuss certain traits in non-Europeans, both savage and civilized.

A. Predominance of the extravert disposition in savages and its reinforcement by social forces. Some peculiarities of savage behaviour best explained in terms of the easy dissociation which is one expression of this disposition.

B. The unconscious among savages. Methods available for its study. Some results and the questions to which such study suggests an answer.

Dispersion *versus* convergence.

The motivation of certain savage customs and beliefs and the extent to which these can be explained by the reasons adduced by psychologists for certain neurotic symptoms.

A. With regard to the predominance of the extravert disposition among savages, I have discussed this matter in a recent address to the Royal Anthropological Institute[1] in which it is indicated that this conclusion follows not only from the general behaviour of savages but from the relative ease and frequency with which many of them enter into states of dissociation. If this be granted, as it seems it must, the next step should be to consider how far this quality of facile dissociation is due to causes intrinsic to the organism, causes which might be termed neuronic, and how far it is to be attributed to extrinsic, *i.e.* social, conditions. In favour of extrinsic conditions is the different social reaction among ourselves and among savages; among ourselves dissociation, while arousing pity, may lead to the asylum and except occasionally does not lead to a fuller life; among savages it almost certainly leads to honour and social advantage. Further, although the absence of privacy in savage life (and it is absent to a degree which in this country, outside the barrack room, it is hard to realize) will probably tend to favour extravert tendencies, yet I believe that no one who has had much personal contact with savages will consider that the main causes of dissociation are not intrinsic, and this certainly is the view which will be taken in this paper. Granted that this ease in dissociation is in the main neuronic it seems to supply the answer to a question that must have suggested itself to everyone who has lived among and with primitive peoples; how is it that savages after carefully enacted, long drawn out, and highly specialized ceremonies, so often can give no other explanation of the ceremony itself or of some of its dramatic incidents than "Our fathers did this..."? Or the answer to the white man's question may be an obvious rationalization, perhaps one that the native himself evidently scarcely believes. It seems that the explanation is somewhat on the following lines:

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[1] "Anthropology and Psychology," Presidential Address, 1924.

Priests and medicine men are adepts at passing into a condition of dissociation, so in some peoples are many commoners, while among the Veddas it is probably true to say that everyone passed into a dissociated condition from time to time. And since the common explanation of dissociation is possession by an ancestral spirit, it is easy to understand how actions performed or instructions issued during or having reference to the dissociated state acquire the highest validity so as to be carried on consciously from generation to generation. It may well be that many of the ceremonies and customs which puzzle anthropologists and for which the savage has no explanation have begun in this way, that is, they have sprung directly from the unconscious of a leader or medicine man, or even of an ordinary member of the community[1].

Moreover, as suggested by Dr Ernest Jones, it is possible "that the conscious thinking of savages is more directly and extensively influenced by unconscious factors than in civilized peoples[2]." Perhaps it might be said that the Unconscious passes into the Conscious more readily than among the White races, and this easier communication would certainly seem to hold of our own neurotics, in whom, as in the more primitive peoples, there is broadly a greater tendency to exteriorization or dramatization.

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[1] Howitt notes that among the Wiimbaio (belonging to the Yuin sub-group) his informant Tulaba was visited during sleep by his father's brother, who taught him formulae against sickness and other evils (*The Native Tribes of South-East Australia*, 1904, p. 437). A custom so originated may be of trifling or of considerable importance and persist for a short or long time. The *tahaka* figure of the dance *maginogo* (Seligman, *Melanesians of British New Guinea*, pp. 157–8) may be cited as an example of an unimportant custom which has lasted through a number of generations.

[2] "Psycho-Analysis and Anthropology," *Journ. Roy. Anthrop. Inst.* 1924, p. 56.

The little I have been able to learn of dramatization among our own psychotics and psychoneurotics has impressed me with the comfort and sense of security which dramatization brings with it. If it be permissible to predicate this effect for savage peoples it would explain the quality of earnestness they commonly bring to their ceremonies, as well as the prolonged duration and their delight in such long drawn out and complicatedly dramatized rites as the Australian *intichiuma* (ceremonies having for their purpose the increase of the food supply, animal and vegetable)[1]. It must, moreover, be remembered that the *intichiuma* are avowedly a repetition of the ceremonies performed by the *alcheringa* ancestors with a magic far more powerful than that of their descendants, *i.e.* the *alcheringa* was the time of unlimited wish-fulfilment.

The broadened communication between the Conscious and the Unconscious would also go well with that heightened sense of volitional power which psycho-analysts have called the Omnipotence of Thought. I would suggest that dramatization be regarded as an expression of this heightened volitional feeling, a primitive 'make it so,' a rather special example of the Omnipotence of Thought.

If this is correct the root of sympathetic magic would be the same whether the magic be imitative and dynamic in the sense that a procedure imitating the desired result be practised, or static as in the doctrine of signatures where the very resemblance invoked would be considered an exteriorization (or dramatization) of the quality and result desired.

B. One of the most important questions alike for the anthropologist and the psychologist is to determine whether fundamentally identical beliefs and technical processes when found in parts of the world remote from each other, or from any obvious common source, are necessarily to be considered as having been carried from a single centre, or whether we may refer their existence, apparently unrelated in space and time, to the independent response to the action of similar environment or needs on different peoples. The extreme dispersionists entirely deny such independent origins. But while admitting that every instance must be judged on its own merits I hold their assertions to be far too sweeping, and that convergence has taken place in the sphere of human activities just as it has in structural development among animals[1].

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[1] The *intichiuma* ceremonies of the Arunta have been described in detail by Spencer and Gillen (*The Arunta*, 1927, chap. VIII), that of the Witchetty Grub totem occupying six pages. The following précis by Haddon (*Magic and Fetishism*, 1906, p. 42) is quoted in order to give some idea of the dramatic content of these rites: "The...ceremony is performed at a special cave, where lies a large block of quartzite surrounded by small rounded stones. The former represents the perfect insect, and the latter its eggs. The headman and his associates tap the large stone and chant songs, the burden of which is an invitation to the insect to lay eggs; the headman strikes each man with one of the small stones, saying, 'You have eaten much food.' Later they go to a large rock which they tap, and invite the animal to come from all directions and lay eggs. After various symbolic ceremonies they enter a long narrow booth made of bushes, which represents the chrysalis case from which the perfect insect emerges, and there they sing of the animal in its various stages and of the sacred stones."

I believe that the study of the Unconscious will help us in this matter, since if it be found that the results of the action of the unconscious, its products, if I may call them so, are identical in peoples of unrelated races and in varying stages of civilization, it will seem reasonable in those instances in which no presumption of immediate or mediate contact exists to attribute an independent origin to similar beliefs and simple technical processes in such unrelated peoples.

Broadly speaking we may study the unconscious by way of:

(*a*) Dreams.
(*b*) Neuroses and other abnormal conditions.
(*c*) Myth, legend and ritual.

The evidence which might be forthcoming from an examination of the last class, viz. myth and legend, must be ignored in this paper, since every instance would demand prolonged scrutiny from the point of view of the possibilities of transmission; moreover the evidence can never be absolutely conclusive. There remain then for consideration data coming under the first two headings.

I do not think anyone will suggest the possibility of the transmission of particular dreams from people to people; if then the same dream is found among unrelated peoples we may conclude that their Unconscious is fundamentally similar, while if we find the same latent content translated into an identical or fundamentally similar manifest content it may fairly be argued that no racial or culture contacts can be responsible for these identities.

Such 'type-dreams' do in fact exist, and in the address already referred to some space was devoted to the subject. In that address I examined the distributions and significance attached to the tooth-losing dream, the flying dream, and the climbing dream, in the Old

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[1] The existence of a flying membrane in both squirrels and lemurs, *i.e.* in groups of animals genetically not closely related, affords an excellent example of 'convergence' in the animal world, while the fire-piston studied by Balfour (in *Anthropological Essays presented to Edward Burnett Tylor*, Cambridge, 1907) may be taken as a good example on the technical side of human culture.

World, and although the records collected were limited to Continental Eurasia they are sufficiently numerous and widespread to indicate the occurrence of these dreams among many unrelated peoples and to raise the presumption that they are universal.

It is unnecessary to recapitulate the evidence with regard to the three dreams mentioned, though the theme is sufficiently important and interesting to warrant the examination of another dream, that of raw meat. This dream in the great majority of instances in which it is recorded is regarded as a thoroughly bad dream, presaging bad luck and misfortune, but before discussing the reason for this it will be well to give its distribution so far as it has been possible to discover it.

Europe:

Ireland—Misfortune[1].

Switzerland—A native of Zurich, nurse in an English family "paid great attention to dreams of meat....She was always greatly depressed after one of these dreams as she had a conviction that they brought bad luck[2]."

Greece—Raw meat betokens unpleasantness and grief. Another dream book says that to dream of eating raw meat betokens trouble in business affairs[3].

Galicia—Illness or trouble[4].

Ukraine—With but few exceptions the dream is universally explained as a bad dream, presaging misfortune or sin, 'shame and sorrow.' In the district of Zhitomir raw hashed meat means burial, or misfortune. In the district of Berdichew a woman who dreamed that her dead mother brought her raw meat to cook regarded it as a prophecy of the birth of a dead calf; in this district it is also said that dreams of raw meat might be a message from the dead, who ask for a mass and dinner to be given (especially to the poor) in their honour[5].

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[1] In one of his novels Mr Horace Annesley Vachell introduces the incident of a character dreaming of butcher's meat, which supposedly presages misfortune. He has informed me that he learnt of the superstition from an Irish family, who believed in its truth.

[2] Communicated by Mr G. R. de Beer.

[3] Communicated by Prof. W. R. Halliday.

[4] Madam Katherine Hrushewsky sends me the following example: A cook about to leave her employment says that she has been calumniated by the proprietor of a small restaurant. When told that there could be no reason for abuse since the restaurateur had never been her enemy the woman explained that she had been dreaming of a heap of raw meat that she was to prepare for dinner in the kitchen of the restaurant, and she was quite ready to take revenge on the restaurateur for this imaginary offence.

[5] Communicated by Madam Hrushewsky.

Africa:

Gold Coast (Northern Territories)—"To dream of raw meat (it does not matter if it has been cut up or not, nor what sort of meat) is a sure sign of death[1]."

Nigeria (Northern Provinces, neighbourhood of Zungeru)—"Dreams of this type presage the death of friends or acquaintances: *as distinguished from blood-relations*. They are most dogmatic on this distinction[2]."

Tanganyika Territory (Urwira)—To dream of meat cut up ready for cooking is bad. It indicates a death, for the meat so cut up suggests a *matango* (funeral feast)[3].

Asia:

Borneo—According to Murut belief "death would soon occur in the village[4]."

Achin—"To dream of meat means misfortune[5]." This is so strongly believed that special precautions are taken to avoid this dream; thus at night "meat must be described as *eungkōt darat* (land-fish), or if the real name be used, it must be preceded by the words 'let no one dream of it to-night[6].'"

China—"Death of a friend or relative[7]."

When citing this dream as presaging misfortune I pointed out that it was entirely unexpected to find that it was so generally recognized as 'bad,' for among primitive peoples generally there is nothing more sought after or more enjoyed than a debauch—if it be permissible to use the word—of flesh. I went on to suggest that the explanation probably was that the most frequently recurring occasions for the consumption of meat in considerable quantities arose in connection with death ceremonies, and I put forward the possibility that there might even be some connection with cannibalism.

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[1] A. W. Cardinall, "Note on Dreams among the Dagomba and Moshi (Northern Territories, Gold Coast)," *Man*, 1927, LIX. Mr Cardinall continues: "I was unable to learn if the death was that of the dreamer or of one of his close relatives. My informants, there were four of them, on separate occasions, were silent on the point and said they did not know. I venture to think the death of the dreamer was indicated, because I told the dream as if it were my own."

[2] Communicated by Dr M. Cameron Blair, who adds: "This type of dream seems to be very common. The people concerned are, all of them, meat-eaters; but they are not cannibals, nor have they ever been so far back (about 1000 years) as their history, together with their antecedent traditions, extends."

[3] G. W. Hatchell, "Some dreams from Urwira (Ukajala), Tanganyika Territory," *Man*, 1927, LX.

[4] Information obtained by Mr G. C. Wooley of the British North Borneo Civil Service.

[5] C. Snouck Hurgronje, *The Achehnese*, Leyden, 1906, XI, p. 45.

[6] Hurgronje, *loc. cit.*

[7] Communicated by Mr Y. R. Chao of Cambridge, Mass., U.S.A.

That the first of these suggestions has a considerable degree of validity seems indicated by Mr Hatchell's discovery among the people of Urwira (Tanganyika Territory) already quoted: "To dream of meat cut up ready for cooking is bad...for the meat so cut up suggests a *matango* (funeral feast)."

It is true that this is the only example in which the connection is directly stated among primitive peoples, but if the symbolism be connected with death repression with persistence of the affect is what might be expected. Moreover, the same symbolism appeared to exist in a European psychotic observed by Dr E. J. Kempf of New York. In this instance a Russian peasant woman had since childhood suffered from horrifying visual and auditory hallucinations of her dead grandfather, who urged her to come to him; she twice attempted suicide, and complained of dreaming of meat 'all cut up,' and of two men bearing a dead woman away in a coffin, who were also coming for her[1].

Although the above examples may be taken to prove that the idea of death is at the root of the generally recognized latent content of the raw meat dream, this does not necessarily imply that all connection with the tendency to regard human flesh as food is absent, and it will be remembered that psycho-analytically vegetarianism is regarded as an expression of repressed cannibalism. I would therefore urge the exploration of the motives which induce the habit of vegetarianism, and tentatively would suggest that the two following records may not be without significance:

Dr Kempf writes that one of his patients, a young woman, particularly fond of meat, was a vegetarian by way of escape, because it gave her a peculiar erotic feeling of eating people when she crushed the meat fibres. When asked 'Who are the people?,' her spontaneous association was 'Grandmother.'

The second record is given me by a British psychiatrist of considerable experience: "I am not a vegetarian, though I have tried to be...and am rather fond of carving, but I have a feeling of disgust if I have to disarticulate a joint when so doing. I have always associated this with a recognition of the similarity to human joints and a feeling of cannibalism...we adopted vegetarianism at home...when [my wife] found that I was not well she, on her own initiative, ordered in meat for me;

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[1] Edward J. Kempf, *Psychopathology*, 1921, pp. 83–5.

this was all very well so long as she gave me chops or steaks, but as soon as she put a joint before me the old feeling of disgust returned." This record, suggestive as it is, would perhaps be more significant if the subject were not a medical man; it is at least plausible that the disgust felt when disarticulating a joint may be linked with old dissecting or post-mortem room experience[1].

Before leaving this dream it must be pointed out that there are exceptions to the rule that it is regarded as ominous, though these scarcely invalidate what has been written above. I have only met with two instances, both in European peoples. In the first instance, from Greece, where "to see various kinds of raw meat hung up in a market indicates abundance and prosperity[2]," the manifest content is such as to produce in the conscious mind ideas associated with fortune and prosperity. In Ireland "to dream of fresh meat means a birth," because of the rejoicing and festivity at the birth of a baby; it is added that chops are "given to the woman about two days after she has borne the child[3]." Here there is a rudimentary process of analysis, and the association is superficial and one that might readily occur to the conscious mind.

Before leaving the subject I may be permitted to quote an example already published of a dream taking place under almost experimental conditions, in which a universally felt emotion has picked out the same symbol to express itself among two unrelated races, who, however, share a particular feature in their educational system, namely, that form of anxiety dream limited to the professional classes in which the dreamer has again to present himself for his final examination. The frequency of this dream among ourselves is well known, the result of recent inquiry shows that it also occurs among Chinese of the corresponding professional class, *i.e.* among the *literati*, a group of men whose attainment to office has depended on the results of an even more strenuous system of examination than that undergone by our own professional men and civil servants[4].

[1] Since writing this I have heard from Dr Roheim that one of his patients has no objection to eating meat as such, but has feelings of disgust when confronted with food in which the articulations are obvious. Association carried this back to a childhood memory of the jerky and fluttering movements of chickens killed for the table. It is poultry with their many joints (especially the wings) that arouse the feeling of disgust.

[2] From a popular dream book.

[3] Communicated by Miss Eleanor Hull from one of her folklore informants, a Cork man, who had found the same belief in Galway.

[4] This is so interesting and so much to the point that it seems well to give the evidence for the above statement. Mr X. L. Yih, of the School of Oriental Studies, first told me that

It would seem that the occurrence of type dreams, especially the examination dream, in which the dream is determined by similar features in the environment of two widely differing races, is an important fact affording strong evidence against the necessity to postulate transmission in the distribution of similar beliefs and simple technical devices in widely separated parts of the world.

The motivation of certain widely distributed customs and beliefs among primitive peoples may next be examined.

There seems no doubt that the motivations put forward by psychoanalysts to explain certain neurotic complexes hold good for the individual; the important matter from the anthropological standpoint is to determine how far these motivations are responsible for certain customs and beliefs among savages, *i.e.* how far they are valid for whole groups. My contribution towards the solution of this difficult question will, in the first place, take the form of an examination of the widely spread practice of removing two or more of the incisors, though discussion —which must necessarily be brief—will be limited to two areas, viz. to the northern and eastern portions of Africa and to Australia.

Evidence derived from soldiers suffering from war-neuroses, including one case which I was able to study at first hand, indicates that in these men the tooth-losing dream was symbolic of the fear of damage to the genitals in battle, while there is evidence that this dream in the male in peace time expresses fear of castration or impotence, sometimes I believe not entirely unconscious. The question to be examined is how far is the knocking out of the lower front incisors in scores of African tribes to be put down to the same fear, in other words, is it a dramatization, and so an abreaction of the fear?

Among ourselves one explanation of the equation, genitals = tooth, is the relatively simple displacement upwards, but that the matter is not basicly so much a simple substitution as a definite connection is shown by such stories as one recorded by Giraldus Cambrensis, who wrote in the twelfth century, which also indicates that the recognition may exist in the conscious mind of Europeans.

educated Chinese do have this dream, while Dr Chi Li, of the Nankai University, kindly sent me the following answer to my questions: "To dream that one has to pass the examination again after he has already passed it occurs quite commonly among the *literati*. I have obtained this information from my father, being himself a *literatus* and having passed many stiff examinations in his younger days. But he could not give me any concrete description of any well-remembered dream that had actually occurred. He only assured me that this type of dream is very common."

> The lord of that place [Chateau-roux in France] maintained in the castle a man whose eyes he had formerly put out, but who, by long habit, recollected the ways of the castle and the steps leading to the towers. Seizing the opportunity of revenge, and meditating the destruction of the youth, he fastened the inward doors of the castle, and took the only son and heir of the governor of the castle to the summit of a high tower, from whence he was seen with the utmost concern by the people beneath. The father of the boy hastened thither, and, struck with terror, attempted by every possible means to procure the ransom of his son, but received for answer that this could not be effected but by the same mutilation of those lower parts which he had likewise inflicted on him. The father, having in vain entreated mercy, at length assented, and caused a violent blow to be struck on his body; and the people around him cried out lamentably, as if he had suffered mutilation. The blind man asked him where he felt the greatest pain. When he replied in his reins, he declared it was false, and prepared to precipitate the boy. A second blow was given, and the lord of the castle asserting that the greatest pains were at his heart, the blind man expressing his disbelief, again carried the boy to the summit of the tower. The third time, however, the father, to save his son, really mutilated himself, and when he exclaimed that the greatest pain was in his teeth: "It is true," said he, "as a man who has had experience should be believed, and thou has in part revenged my injuries. I shall meet death with more satisfaction, and thou shalt neither beget any other son nor receive comfort from this." Then precipitating himself and the boy from the summit of the tower, their limbs were broken, and both instantly expired. The knight ordered a monastery to be built on the spot for the soul of the boy, which is still extant and called De Doloribus[1].

It may be then that the substitution of the teeth for the genitals is a secondary idea, comparable perhaps to that further substitution which explains the manifest content of the tooth-losing dream as being not the death of the dreamer (*i.e.* of oneself) but the death of a near relative. We may perhaps check the idea that the removal of teeth is motivated by what among ourselves would be the neurotic fear of castration by considering the distribution, in areas where both occur, of the customs of avulsion of teeth and of circumcision. There are two great land masses in which both these customs exist, namely, Africa and Australia. In Australia the distribution of the two customs has been much studied, while of Africa, especially its north-east central portion, I have personal knowledge. In this part of Africa there are large areas in which (*a*) circumcision exists without removal of the teeth, and (*b*) removal of the teeth (lower incisors) without circumcision. The former area includes the Egyptians, all the Hamitic tribes of the Eastern Desert and Eritrea, Abyssinia (perhaps not quite the whole) and Somaliland, as well as the Arab tribes; the latter the Nilotes, their southern and eastern neighbours the Bari and the Lotuko-speaking tribes, the Nuba of Kordofan (apart from recent Arab influence in the north) and most of the tribes on the west bank of the Nile extending southwards from Kordofan to the neighbourhood of Lake Victoria. Thus circumcision extends from the Mediterranean to the equator, and tooth extraction from the neighbourhood of Khartum to the Great Lakes, the tooth-extracting area lying on the whole south and west of the area of circumcision, and there does seem to be a definite tendency for one rite to exclude the other. That this rule is not invariable is indicated by the practices of a number of tribes of mixed Negro-Hamitic descent, which I have elsewhere termed half-Hamites. Yet because the tribes practising both rites are but a fraction of the population of either of the two main groups, and because they inhabit a border territory (largely Kenya Colony) where Hamite and Negro have mixed, it seems legitimate to regard their habits as exceptional, to be explained historically by the known relatively recent mixture of the two stocks—Negro and Hamite, each keeping their customary mutilation—which have fused to form them, the persistence of both mutilations in the half-Hamites being perhaps in a sense analogous with that 'over-determination' which so often takes place in dreams.

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[1] Giraldus Cambrensis, *The Itinerary through Wales*, bk. I, ch. XI, transl. Sir Richard Colt Hoare. The Itinerary was made in 1188 and the account of it written not long after; I am indebted to Mr E. N. Fallaize for drawing my attention to this passage.

In Australia there is the same suggestion of two great territories with an area of overlap, for the great group of south-eastern tribes do not circumcise, but the extraction of the front teeth is an essential part of their initiation ceremonies, while among the central tribes, *e.g.* the Arunta, circumcision is obligatory and the extraction of teeth optional (except for certain groups connected with rain), and in north-west central Queensland there is an area of overlap in which both circumcision and avulsion of the teeth are practised, the latter, I believe, being regarded as the less important of the two operations[1].

In a great part of the vast area of Australia in which a penile operation is practised, there is the tendency to increase the severity of the operation, *e.g.* by subincision, and in what I have written above 'circumcision' is used to indicate any operation on the penis, thus including all varieties of procedure in which the foreskin is concerned, as well as subincision[2].

[1] The mutually exclusive relationship of tooth extraction and circumcision was noted as long ago as the first half of last century by the explorer Eyre.

[2] With regard to penile operations, while I do not wish to express an opinion as to the validity of the psycho-analytic interpretation of these ceremonies, *i.e.* that youths were originally castrated as a punishment for incest in connexion with the nuclear complex (an operation which performed at the dawn of man must have had an enormous mortality) it is worth noting that in Australia there is a tradition of a mode of circumcision so barbarous that the boys died under it (Spencer and Gillen, *The Native Tribes of Central Australia*, p. 402) and also of the punishment of a man who castrated and killed a youth who ought only to have been circumcised (*op. cit.* p. 398; cf. also J. G. Edge, quoted by Roheim in *Australian Totemism*, p. 177).

A further hint as to the equivalence of the tooth and the foreskin is suggested by the practices of a number of tribes of the Northern Territory, *e.g.* of the Gnanji and the Binbinga, both practising circumcision (and subincision) with optional avulsion of the teeth. Among the Binbinga the blood from the circumcision (but not the foreskin) is collected and buried in the bank of a pool, with the avowed object of increasing the supply of edible water lilies[1], while the Gnanji treat the extracted teeth in the same way and for the same object[2]. The Anula observe this practice with regard to the foreskin, though the reason for this is not given[3].

Further, as pointed out by Sir James Frazer, there is some reason to suppose that in Australia both foreskin and tooth are regarded as agents of reincarnation[4].

Incest is another matter in which, as it seems to me, the conduct of European neurotics and the behaviour of certain savage and barbaric customs may be equated with each other. I understand that the fear of incest, at least in the unconscious, is one of the commonest fears of psychoneurotics, and it may be supposed that it not infrequently comes through into the conscious. In savages the fear is frankly in the conscious, as is shown by the precautions taken to avoid the possibility of incest, especially by the various observances between members of opposite sexes which the anthropologist sums up as 'avoidance.' If it be agreed that the communication between the unconscious and the conscious is freer in savages than in Europeans then there is little to be surprised at in this, and it is easy to understand such prohibitions as that which forbids brother and sister to come face to face or to speak to each other after puberty (New Caledonia) or a brother to come in contact with any part of his sister's bed (Baganda). Such observances also seem to support a suggestion made by Dr Ernest Jones that the fear of incest in the savage is greater than in the European who, phylogenetically, may be regarded as having outgrown this fear to a great extent.

Additional evidence of the intensity and nature of the fear of incest among savages is offered by its common punishment, death, and by the point of view which dictates this penalty, *e.g.* among the Ashanti, who hold that by committing incest the guilty parties have brought death upon their fellows, it being expected that their sin will be followed by a number of deaths in the community[1].

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[1] Baldwin Spencer and F. J. Gillen, *The Northern Tribes of Central Australia*, 1904, p. 367.

[2] *Op. cit.* pp. 594–6.

[3] *Op. cit.* p. 372.

[4] *Totemism and Exogamy*, 1910, IV, pp. 180–4.

Now, even apart from cases which I suppose exist of psychotics with a morbid fear or desire of death, motivated by the idea of punishment for the incestuous wish or for its believed realization, I am prepared to accept the psycho-analytic finding as to the cause of the fear of death in many instances. And doing so I would lay considerable stress on an instance occurring in a primitive people in which a particular fear of death, called by a special name, is stated to be the result of committing incest. Among the Ba Kaonde the word *chimalwamalwa* signifies "the fear of death caused by committing incest." It is related that the first man who committed incest died for his act, thereby proving to future generations that this was indeed a deadly sin. "If a man commits incest now he gets *chimalwamalwa* (the fear of death)...," the explanation being that he has roused the spirit of the first incest sinner[2].

CONCLUSIONS.

(1) The generally extravert disposition of primitive peoples to which I have drawn attention elsewhere is regarded as a neuronic, *i.e.* congenital, trait, although it is admitted that most primitive environments are socially favourable to this disposition.

(2) A parallel is drawn between the satisfaction felt in dramatization by our own pyschoneurotics and by primitive peoples, and the suggestion is made that in both this satisfaction is an example of the process which psycho-analysts call the omnipotence of thought, by which also imitative magic is to be explained.

(3) A study of the dream of raw meat is used to show the wide distribution of type dreams among peoples not closely related genetically, and an explanation showing persistence of the affect is offered as to

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[1] Information for which I am indebted to Captain R. S. Rattray.

[2] F. H. Melland, *In Witchbound Africa*, 1923, pp. 143–4. The association of incest with the idea of death seems to exist even in such instances as the following (among the Ba Thonga of Portuguese East Africa) where the act is undertaken ceremonially and of set purpose in order that its performer may rank with a deliberate homicide. A hippopotamus hunter will spend a long time studying the animals; when "he sees that the propitious moment has come and is ready to undertake hunting operations lasting a month, he first calls his own daughter to his hut and has sexual relations with her. This incestuous act, which is strongly taboo in ordinary life, has made him into a 'murderer': he has killed something at home; he has acquired the courage necessary for doing great deeds on the river" (H. A. Junod, *The Life of a South African Tribe*, 1927, II, p. 68). It will be remembered that the punishment for deliberate murder is death (*op. cit.* I, p. 442).

why this dream should be regarded as 'bad.' Such type dreams are regarded as indicating the probability of the independent origin among different peoples of similar or identical beliefs, customs and technical processes.

(4) Comparison of the fear of castration in neurotics and of ritual circumcision on the one hand, and of the avulsion of teeth in Africa and Australia on the other, suggests that the motivation of the latter may be due to the same fear (in the unconscious) as the former, while the, to us, exaggerated fear of incest of primitive peoples, with their ceremonial devices for avoiding this sin, is considered to be the equivalent of the incestuous fears of European psychoneurotics.

(*Manuscript received* 25 *January* 1928.)

SOCIAL CONSTRUCTIVENESS[1]. I

BY F. C. BARTLETT.

McDOUGALL thinks that "we are justified in assuming in man an instinct of construction," but he says very little about it. "The playful activities of children," he asserts, "seem to be in part determined by its impulse; and in most civilized adults it still survives, though but little scope is allowed it by the circumstances of the majority[2]." He believes that the impulse has important social expressions, but fails to develop these, merely remarking that "the principal social effects of the instinct of construction are produced by the necessity for co-operation in works of construction that surpass the powers of individuals, especially architectural works[3]." Similarly Rivers writes of a social constructive instinct, but does not make much use of it, indicating only that "its most complete and striking developments are connected with the parental occupation of nest building, and with the social ends of the honey bee[4]."

It appears to me a matter of interest and perhaps of considerable importance to try to see how this constructive tendency, whether we call it instinctive or not, actually operates in society; what are its main stimuli; what kinds of results it produces and how it produces them; and what is the relation of the final effects produced to individual aims and purposes.

We know that any social group that can be studied possesses an organization, institutions, forms of material culture and functions of a considerable degree of complexity, and that these are in many cases peculiar to itself and are the characteristics which we use when we wish to distinguish it from other groups. We know in many cases that the elements of these complex forms came to the group ready-made from some outside source. But within the group they have developed their own peculiar forms. Can we say anything definite about the social and psychological processes by which this development is effected?

Very broadly speaking two opposing views may be put forward.

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[1] This, and the succeeding three papers were contributed to a joint psychological and anthropological discussion held at Cambridge in March, 1927.

[2] *An Introduction to Social Psychology*, 20th edit. p. 75.

[3] *Op. cit.* p. 279.

[4] *Instinct and the Unconscious*, p. 53.

According to the first, the social group should be looked upon as a sort of receptive mechanism always tending to whittle down anything new which is brought to it into some form which existed before the new material appeared, but never directly responsible for the production of anything new on its own account. According to the other, processes of change affecting a social group, however they may be introduced, are continually being actively shaped into new forms and never merely assimilated to old ones. This may be regarded as a way of stating the old controversy between 'contact'—which leans towards the first view—and 'evolution'—which leans towards the second view—as the key to the explanation of the growth of social structure and functions.

Let us assume that the main stimulus to change in society comes from outside the group which changes. This may not be altogether true, but it is a good starting-point for discussion. Now obviously customs, or institutions, or traditions cannot be transferred from one group to another irrespective of the social conditions of the receptive group. No amount of 'contact' will ever cause a group to take over anything for which there is no fairly obvious setting in its own culture. For much the same reasons, no custom, tradition, or institution ever is taken over by one group from another which does not very speedily suffer important change. The general direction of this change is to make the importation more like the existing culture. In a broad sense the more important and significant, socially speaking, is the importation the more marked are the changes it is likely to undergo. Thus it is safe to say that the similarities of culture which may justly be used as arguments for social contact are always of relatively detached, unimportant and odd details. I think it to be the case that genuine likeness of important elements of culture which have persisted for any length of time points to the view that such elements have grown up independently in the social settings in which they are found.

When a custom is taken over by one group from another, it is subject to change:

(*a*) by assimilation to existing cultural forms;

(*b*) by the dropping out of many elements peculiar to the group from which the custom comes;

(*c*) by retention, in many instances, of details peculiar to the communicating group, but apparently not very vitally connected with the central custom which is adopted.

Further, these three processes can all be treated as direct results of a social situation, and independently of a study of the attitudes of the individuals in the groups concerned. From all these changes there may result a custom which is not found anywhere else than in the receptive group in question, but they are not genuinely constructive changes, for they are achieved by a blend of assimilation, omissions, and retention of odd elements.

In every case however we have to try to treat the social group as possessing, not merely a social structure built up in its past but also a general trend of development. This trend of development need not be, so far as I can see, adequately represented in the mind of any individual member of the group, but it is nevertheless a genuine factor determining social change within the group. Thus when a custom comes in from outside it is changed, not only by assimilation, simplification and retention of relatively unimportant elements, but positively in the direction along which the group happens to be developing at the time at which the custom comes to it. Since this direction may perhaps not be fully realized—to say nothing about its being fully formulated—by any member of the group, the influence which gives a positive bias to changes in a certain direction which the group is moving towards, may fairly be called *social*. Moreover the general functions of this influence are certainly constructive, and can be seen particularly in the welding together of elements coming to a group from diverse sources and having historically very diverse significance.

This is what I wish to call social constructiveness. It is a characteristic reaction towards imported elements of culture adopted inevitably by all strong and vigorous groups. It means that imported elements suffer change both in the direction of existing culture and along the general line of development of the receptive group. It means that while it may well be the case that the stimulus to social change comes in the main from social contacts, important social forms may genuinely grow up within a group. Coupled with what I said in the earlier part of the paper I think it goes far to justify the view that striking similarities in important elements of culture are an argument rather for independent origin than for transmission of such elements from one group to another. Probably the main way in which 'social constructiveness' is exercised is that all incoming cultural elements, whatever their origin, having to do with the same general sphere of life, will be dealt with by the activities which determine the trend of development of the receptive group in relation to the sphere of culture involved. Consequently they will be built together and worked up into forms the details of which come from varied sources. This is what makes it necessary to call the whole process constructive.

Clearly we want to know a lot more about the precise mechanism of the process which I have characterized in general terms only. As I have said, I believe that the main underlying basis of this type of change is truly social and cannot be expressed in terms of individual psychology. But no doubt individuals of a dominantly constructive habit of mind do play a large part in the mechanism by which this social tendency finds concrete expression.

SOCIAL CONSTRUCTIVENESS. II

By J. T. MACCURDY.

Mr Bartlett has given us a clear description of certain phenomena which he has termed 'social constructiveness' but he has—perhaps wisely—refrained from defining 'social' and 'constructiveness.' He says in his conclusion, "Probably the main way in which social constructiveness is exercised is that incoming cultural elements whatever their origin, having to do with the same general sphere of life, will all be dealt with by the activities which determine the trend of development of the receptive group in regard to the sphere of culture affected. Consequently they will be built together and worked up into forms the details of which come from varied sources."

This statement goes beyond mere description for it assumes the existence of "activities which determine development." This assumption, natural as it may be in view of the data which Mr Bartlett summarizes, is one that cannot be adopted lightly for it involves matters of broad theoretic import. As brick is added to brick a house is constructed but we know the process is not passive. There is a builder. When grain is added to grain of sand a sandbar results; but there is no programme being followed, so we speak rather of the deposition than of the combination of a sandbar. Mr Bartlett argues from the existence of a trend of development to the existence of an activity which provides the direction. Where does this activity reside? Is it in the minds of the individuals forming the group, representing merely the summation of the actions of unit minds, or is the group as such, an entity distinct in its functions from the mere addition of its individuals. Mr Bartlett holds that it is, for he says: "...I believe that the main underlying basis of this type of change is truly social and cannot be expressed in terms of individual psychology."

At first blush this last would seem to be rather a startling statement for the prime material of psychology is the individual man or animal and our nomenclature must therefore be derived from the study of individuals. If our psychological terms cannot then be applied to phenomena of group behaviour, it would seem to be difficult to dismiss these psychologically at all. What I suspect is that Mr Bartlett is assuming constructiveness in the human individual to be a conscious process and he hesitates to ascribe consciousness to a group. This is the one point which I wish to discuss, which may be put in the simple query: Can there be mental activities exhibiting a plan, a programme, a utilization or discarding of material in virtue of a given aim or direction without the objective being consciously entertained?

I believe that this is a polemical question among psychologists very largely because our terms are all taken from everyday speech and therefore are dowered with a variety of meanings which will persist in spite of our efforts to make the terms scientific, *i.e.* precise. The layman uses 'consciousness' in at least two different senses. He speaks of his being conscious of his thoughts or of the environment in the sense of subjective awareness, a knowledge of some process going on within his own mind and, on the other hand, of his being conscious when he merely reacts to a stimulus. This latter meaning comes out most clearly in its negative usage. We say a man who has been stunned is unconscious and that he returns to consciousness when he responds to some stimulus as, for instance, withdrawing from a pinprick, although there is not the slightest evidence either from contemporary comment or subsequent memory that the pinprick set up any mental process of which he is subjectively aware. In consequence of this double usage psychologists are prone to the error of ascribing conscious direction to activities which merely demonstrate specific reactability. By specific reactability I mean a discrimination of stimuli in terms of their suitability for the reaction in question.

It is possible to describe such discrimination in language that inevitably suggests self-consciousness or, alternatively, in a purely objective manner. If an organism shows a failure of response to certain stimuli, while it responds to others or responds differently to the same stimulus at different times these variations may be described in terms of the availability of the stimulus for the attainment of the end result of the reaction and thus be regarded as purposeful, which is a term belonging properly to introspective psychology. Or the description may be made in words which carry no such implication. We then say that, when certain functions or integrations of functions are operating, the thresholds for various stimuli are selectively altered. In the latter case we use a vocabulary applicable to observations either in the physiological or in the psychological sphere.

In the minds of many people the demonstration of the quality of the reaction being determined by the nature of the result of the reaction rather than by the nature of the stimulus implies purpose and, as inevitably, consciousness, in spite of the violence this does to any significant definition of consciousness. For instance, one reads sometimes of the 'consciousness' of the isolated spinal cord in the writings of an observer who has discovered teleological functions in spinal reflexes. The scratch reflex, for example, cannot be elicited by a single electric shock no matter how powerful, but if the shocks be repeated or distributed so as to resemble the movements of a parasite the reflex appears. The spinal cord is then held to have 'perceived,' having perceived to have a mind, and, similarly, to have consciousness. Or, a stimulus may produce flexion in a leg that is extended, while the same stimulus sets up extension in a leg that is flexed. Owing to the potential existence of a stepping reflex the limb 'wants' to go on to a movement in the direction opposed to its immediate posture. Again, it is easier to elicit the extensor thrust reflex in a hind leg of a spinal dog when the leg is already flexed, *i.e.* in a position naturally preceding a leaping movement. One can say that the spinal cord is thinking of taking a leap or one can be more objective and speak of a lowered threshold for an extensor thrust reflex when that reaction is potentially present. This is an extremely simple example of the type of process which, in its more complicated form, Mr Bartlett speaks of as activities directing a line of development in a group of people. We may read consciousness as legitimately into one of these phenomena as into the other.

If we are to use consciousness in the sense of mental and see mind wherever purpose seems to be operating, then physiology might well be termed only a branch of psychology. For it seems to be the case that whenever the physiologist studies a bodily reaction that cannot be duplicated in the physical or chemical laboratory he is dealing with phenomena that are most adequately described in teleological language. The vitalists, noting the failure of materialistic explanations, invoke an 'entelechy' which obligingly makes good the deficiency. It may be that they are right, but such an animistic conception should not be adopted except as a last resort.

It seems to me that we are on safer ground if we admit that vital processes do differ from purely physical and chemical ones and then devise a vocabulary for the study and correlation of functions *qua* functions. We will then admit teleology as a guiding principle in the direction of reactions at all levels of biological function without ascribing this activity either to an animistic agency or to consciousness. We shall then find that wherever an integration of functions occurs one characteristic of the integration will be a low threshold for stimuli pertinent to its reactions and a utilization of only such elements of the stimulus as are pertinent thereto. Herd animals form a group which then behaves as a unit. In our new vocabulary the functions of the individuals are integrated together creating a new function. The individual then, while with the group, has a low threshold for stimuli arising within the herd and so responds in favour of group movements rather than in favour of individualized response to the environment. This makes of the group an organism, which is a functional, although not an anatomical, unit. Startling though this statement may seem to some, the idea is an old one. Some years ago Von Uexküell argued that the species is an organism and should be regarded as such by biologists. But long before him Darwin not only had the same conception but made it the basis of his evolutionary theory. We find in his notes that his theory of the origin of species was derived as an analogy to the reproduction of individuals: as parent gave birth to child, that was similar in some respects although not in all, so one species could give birth to a similar but not identical one.

But, it might be argued, the bond uniting the individuals in a group being plainly not physical must be mental; this social organism is therefore obviously a psychological being and why not conscious? To this I have to answer that it is conscious or not according to the definition one places on consciousness. If by that term one means self-consciousness, then the 'group mind' may be said to have consciousness only if it can be demonstrated that it is aware of its activities as such. In the individual man we can deduce consciousness only from such behaviour as must be determined by thoughts, *i.e.* by processes which are purely internal and cannot be accounted for as responses to stimuli that follow habitual lines. When a man performs some action that is determined, not by an immediate stimulus, but by an apparent prevision of circumstances existing for the moment only in imagination, then we are justified in saying that he has thoughts, because only thoughts can have shaped his actions. Similarly, we could say that a group of people has its consciousness only if its activities were determined by events that had not then happened, events brought into being by the group and to be accounted for only as a means to a more ultimate end. Can our anthropological friends give us any examples of such group behaviour? Naturally a programme of action entertained consciously by leaders of the group must be excluded from any evidence brought forward to demonstrate 'group consciousness.'

SOCIAL CONSTRUCTIVENESS. III

By W. E. ARMSTRONG.

THE culture of a group may be provisionally described as the uniformities of behaviour of its members other than those which are the direct outcome of the inherited mental make-up of the members of the group—in other words, those uniformities of behaviour which are traditional. A more accurate way of putting this analysis is to say that the behaviour of any member of a group may be analysed into three components, which may be called the 'human response,' the 'cultural response,' and the 'individual response.' The cultural response is, necessarily, an abstraction, just as the notion of culture itself is an abstraction. In a situation, exciting fear, the response will be likely to bear some similarity to the response of members of other groups in like situations—this is the 'human' part of the response—an abstraction. There is, also, likely to be a peculiarity in the response, common to members of the particular group, but not to others—this is the 'cultural' response—equally an abstraction. There is, finally, likely to be some modification of the response peculiar to the individual and resulting from the circumstances of his life-history and the particular genetic variation peculiar to him—this is the 'individual' response—again an abstraction, for no response, however exceptional, is without its human and cultural side. This last component of any response of any member of a group is something purely of the nature of accident, though some statements as to the probabilities of variations of response under this head can be made, according to size of group, nature of its culture, and so on. I wish to suggest in this paper that the only changes of culture which can appropriately be called constructive are the result of individual responses—responses, which are, no doubt, usually individual responses to outside influences.

With regard to those changes of culture, due to outside influences, which Mr Bartlett does not call constructive, I think the process may be analysed in the way Mr Bartlett suggests (*a*) by assimilating to existing cultural forms, *i.e.* by minor modifications of existing cultural responses, (*b*) by dropping out of elements difficult to assimilate owing to difference and importance, *i.e.* that are inconsistent with certain cultural responses of the adopting group, and (*c*) by retention of details not vitally connected with the new custom, *i.e.* that are consistent with the other cultural responses of the adopting group.

Mr Bartlett says that, "these three processes can all be treated as direct results of a social situation, and independently of a study of the attitudes of the individuals in the groups concerned." Here I disagree though not vitally and should prefer to say that these processes are independent of individual responses (though these may determine what particular insignificant items are retained) but are directly dependent on the cultural response of the group influenced. This, however, is a difference of analysis which leads to no disagreement of importance, and both modes of analysis lead to Mr Bartlett's conclusion, pushed perhaps a little further, that the closer the likeness of important elements of culture of groups which have long been isolated from one another the higher the probability of independence of origin.

Having dealt with those changes which are not constructive, Mr Bartlett proceeds to point out that a group has not merely a definite culture but a trend of development, *i.e.* it evolves, and "this trend of development need not be (so far as Mr Bartlett can see) adequately represented in the mind of any individual member of the group, but it is nevertheless a genuine factor determining social change within the group." Now, I cannot see how a trend of development can be a factor determining social change, though I do not dispute the trend nor suggest that in order that there should be such a trend it need be in the mind of any individual. It seems to me that a trend of development of any group is the natural result of the existence of a system of cultural responses. Whatever the causes that have led to these uniformities of behaviour, it is clear that they are unlikely to be fully adapted to one another, and therefore cultural responses will slowly or quickly change quite apart from the influence of individual responses and environmental accidents. It does, however, follow, on my analysis, that this process of evolution of culture is a tendency towards a position of equilibrium, having attained which culture would, apart from the influence of individual responses or outside influences, evolve no further or would return on a cycle of changes repeating the old. (The view I am putting implies that, if individual responses and outside influences be ignored and, of course, newly-evolved human responses, a social group is analogous to a material system, and a material system necessarily tends to equilibrium or a cycle of changes.) I suspect that Mr Bartlett's 'trend of development' is something more than this, a mystic principle of evolution, perhaps similar to that adopted by some anthropologists to explain in universal terms the development of human culture. I can see no evidence nor *a priori* necessity for assuming such a principle of evolution of culture, analogous to biological evolution.

Owing to this trend, a custom, entering a group from outside, is changed not only by the non-constructive changes which we have already considered but also by the direction in which the group is evolving. Because this direction of evolution is not realized by members of the group and yet the custom is altered in this way, so Mr Bartlett says, this change is said to be social, and because the change is one of welding together elements coming from diverse sources with diverse significance it is said to be constructive.

The objections, on my view, to calling this constructiveness social are, in the first place, those (for most purposes trifling) which I touched on in dealing with non-constructive changes, namely that it is the cultural behaviour of individuals which is cause rather than institutions which are not causes but fictions to express uniformities of behaviour which *are* causes. The more important objection I wish to urge is that of postulating a causative factor, called a trend of development, which also inheres in the group in the same kind of way in which an institution is supposed to inhere in a group. Further, it seems to me precisely in these cases of so-called social constructiveness that the incalculable individual response becomes of importance and, in so far as this is the case, we cannot speak of social determination even in the analogical sense, which was granted for non-constructive changes.

Passing now to what is meant by the 'constructiveness' of these changes, I may be charged with begging part of the question, for I wish to call those changes constructive in the causation of which the individual response is largely concerned. All I can do here is to put briefly what this view of development means.

In the first place, there are changes of human response, due to that mysterious process of biological evolution, which are enormously important in a very long period but may be neglected in the comparatively short-period problem of cultural evolution. In the second place, there are changes of culture, which may be called non-constructive, which depend on the uniformities of behaviour called 'cultural' and are independent of individual responses. Since independent of individual responses these changes can be studied by the group method and no appreciable error is introduced by assuming them to be 'socially' determined in Mr Bartlett's sense. It is evolution of culture in this restricted sense which is the only proper sphere of study of the evolutional anthropologist. It is interesting to note that contact, giving non-constructive changes, appears to be the only mechanism of development by diffusion, assumed by Elliot-Smith and Perry.

Such evolution of culture determined from within the group, whether or not modified by contact, is a tendency to equilibrium—it is like the running down of a mechanism, rather than the winding up, suggested by biological evolution. Equilibrium is, however, never reached because individual responses, producing constructive changes, are continually at work, some originating within the group, but probably in most cases stimulated by contact from without. This is the historical or incalculable element in cultural development, the only constructive element, and the one necessarily incapable of explanation whether by evolutional, by diffusional, or by sociological methods.

SOCIAL CONSTRUCTIVENESS. IV

By A. C. HADDON.

My only claim to enter this discussion is that of an anthropologist who does not profess to be a psychologist.

Among the primitive peoples I know best there is a definite idea of standardization. As Landtman says of the Kiwai Papuans, "We even meet with a conscious desire among the people to maintain social equality: 'no want one man he go ahead, one man he come behind; better all man he go together'" (p. 167). "Although there exists no class inequality among the Kiwais, yet a natural dissimilarity makes itself felt between different men as regards their social influence, but this depends on personal qualifications and circumstances, and does not entitle a man to any social precedence....Among the influential people are such as distinguish themselves through their eloquence ('he got plenty talk inside'), for the others like to listen to them and follow their advice" (G. Landtman, *The Kiwai Papuans of British New Guinea*, 1927, p. 168).

The standardization is partly due to the relatively small communities in which most savages dwell, there being a more intimate connection between the members of the group as a whole than can obtain in large communities. Even in the latter, owing to social structures, such as clans, sides of a ceremonial house, etc., there are concrete groups, the members of which are strongly knit together socially. All the ceremonies bind the people together by a common sentiment and by uniform action and these are inculcated during the often protracted and always awe-inspiring initiation ceremonies.

Thus there seems to be a definite and conscious desire towards uniformity and solidarity, and doubtless this has a recognized economic and utilitarian basis, and it certainly makes them a stronger people both for defence and attack.

Landtman says, "Sitting close together in a circle, generally in sight of the people, but out of hearing, the 'big men' talk together in low voices, and any one of them may express his views. It seems very rare for opinions to be in conflict in such an assembly. It is, indeed, surprising to what an extent the people's ways of thinking in public affairs run along identical lines, and the same unanimity is apparent when the general opinion is to be put into action. What one man says or does immediately stirs the others to chime in; everybody is at once ready to accept whatever proposition may happen to be put forward with sufficient persuasive power. After the parley, the leader's decision is announced before the assembled people, and they, too, seem always ready to comply with whatever is suggested" (p. 169).

Williams, speaking of the Namau of the Purari Delta of British New Guinea, says: "One inclines to the opinion that the decision is usually in keeping with the wishes of the majority, or rather of that more able and confident section who make their voices heard in discussion" (p. 117). "The simple conduct of the hunting expedition or of a combined fishing with *uru* [a kind of net] gives a good idea of Purari co-operation. There is generally a conspicuous absence of any effective directing agency, though none of individual advice and encouragement; the marvel is the lack of real confusion in any combined movement" (F. E. Williams, *The Natives of the Purari Delta, Anthropology Report* 5, *Territory of Papua*, Port Moresby, 1924, p. 130).

If we admit, then, that the members of a primitive people are so largely imbued with a similarity in their way of thinking about things, in their sentiments and in their action, it requires but a slight indication to cause action in a certain direction, which, from the nature of the case, would appear to be practically spontaneous. When all essentially think and feel alike there is what Dr MacCurdy, in speaking of herd animals, terms "a low threshold for stimuli arising within the herd and so there is a response in favour of group movements." He says, "it may be argued that the bond uniting the individuals in a group being plainly not physical, must be mental; this social organism is therefore obviously a psychological being and why not conscious?...If by that term one means self-consciousness, then the 'group mind' may be said to have consciousness only if it can be demonstrated that it is aware of its activities as such." He asks if anthropologists can "give us any examples of such group behaviour?" and adds, "Naturally a programme of action entertained consciously by leaders of the group must be excluded from any evidence brought forward to demonstrate 'group consciousness.'" I am not aware that there is any evidence among primitive peoples of 'such group behaviour' concerning which Dr MacCurdy seeks information, and the instances I have given are such as he would exclude as evidence to demonstrate 'group consciousness.'

I must confess that I do not understand by what mechanism a 'group' can have a 'mind.' From what I have said it seems to follow that, so far as I am aware, there is no need to hypothesize the mystical conception of a group consciousness and still less that this has a tendency to develop in a certain direction. I consider that the great suggestibility of savages combined with the uniformity of social behaviour needs only the stimulus of an idea from an individual to start combined and apparently unanimous action. The stimulus, however slight, falls upon well-prepared ground.

It also follows that the uniformity of which I have spoken makes for conservatism, and if a people be left alone under stable conditions of life little progress is likely to occur, and that is what has often been said about savages. It must be remembered that we have extremely little accurate and detailed information about savages which embraces a long period of time, and therefore we can form no opinion upon the direction or rate of their social evolution, assuming it to occur spontaneously.

The more intensive studies of primitive peoples does, however, prove the acquisition of new material objects, customs and ceremonies, but in all cases, so far as I am aware, these can be proved to have been introduced from elsewhere. These are adopted in various degrees of completeness according to the needs or aptitudes of the indigenous folk as Bartlett and Armstrong have pointed out. Thus in many cases social constructiveness seems to be reduced to social adaptation.

In Melanesia intermediate conditions between mother-right and father-right have been described, and it was at first assumed that these indicated an evolution from the former to the latter. But now it appears that we must regard these intermediate conditions as a conscious adaptation to new conditions. Thus an immigrant patrilineal people when settling among a matrilineal people would require an integrating social mechanism, and this would naturally simulate an evolutionary series.

Myth-making is one aspect of the social consciousness of the members of a group which is a somewhat complicated problem. As Malinowski has pointed out, "The myth comes into play when rite, ceremony, or a social or moral rule demands justification, warrant of antiquity, reality and sanctity" (p. 36). By means of myths people strengthen their claim to the land and their relative position in the social structure. "Myth fulfils in primitive culture an indispensable function: it expresses, enhances, and codifies belief; it safeguards and enforces morality, it vouches for the efficacy of ritual and contains practical rules for the guidance of man" (p. 23). "Myths form an integral part of the culture. They form the dogmatic backbone of primitive civilization" (p. 38). "The function of myth, briefly, is to strengthen tradition and endow it with a greater value and prestige by tracing it back to a higher, better, more supernatural reality of initial events. Myth is therefore an indispensable ingredient of all culture. It is constantly regenerated; every historical change creates its mythology, which is, however, but indirectly related to historical fact" (p. 125). "Whatever the hidden reality of their unrecorded past may be, myths serve to cover certain inconsistences created by historical events, rather than to record those events exactly" (B. Malinowski, *Myth in Primitive Psychology*, Psyche Miniatures, General Series, 1926, p. 77).

What happens when the myths of two peoples are brought into contact? A reconstruction must take place. The new comers have their mythology which refers to events that may have occurred in former places of residence and may date back to various times—some perhaps of extreme antiquity—at all events it has probably become stabilized and justifies the social conditions of that particular people. But the same applies to the people of the soil, though perhaps not to the same extent. The myth-making spirit is never dead, but applies itself to the new conditions. What the new comers have to do is to regulate by myths their new conditions of life and also the new social conditions that arise from their arrival. Here adaptation is at work or if you will constructiveness, but who will claim that such constructiveness is due to a group mind independent of individual minds or that it takes a trend of development that is beyond the cognizance of the members of the group to whom such an adjustment is of prime importance?

A great many years ago I flirted with the idea whether there might not be an integration of human beings that might have a 'group mind,' and I suspect this is the sort of thing that Mr Bartlett and Dr MacCurdy have in mind. In biology we know of unicellular organisms which have a distinct 'life' of their own. There are forms composed of loose aggregations of cells and others in which the connection is more intimate, such as Volvox, in which bisexuality is apparent; here we have a 'life' of a higher order. The Coelenterates present us with a higher integration and a greater structural and physiological differentiation of the individual cells of the organism, and so on through the various groups until we arrive at the most complex animal of all—man himself. With progressive specialization we get higher organisms and the supremacy of man is ultimately due to his superior nervous mechanism. This progressive evolution is fundamentally due to intenser structural and physiological specialization of the component elements (cells), but the 'life' of the individual is not a mere sum of the activities of the individual cells, and it is manifested in ways which could not be foretold by one who knew all there was to know about the component cells. With increasing complexity the actions of the organism become more individualized and apparently less definitely related to simple stimuli, and eventually the human mind acts in a way that is at present inexplicable.

The question arises whether a group of men may not be a loose aggregation which might be supposed to have a distinct 'life' or 'mind' of its own, and, if so, then assemblies of groups may be supposed to have something similar, but as I have previously said, I do not understand by what mechanism a group of this kind can have a 'mind.' The statement that a body of men, such as a committee, a council, or the like, may come to a conclusion which could not be predicated from a knowledge of the disposition of its constituent members may or may not be true. Such a conclusion or decision is termed a 'group mind,' and thus that group is supposed to be an organism analogous to a simple multicellular organism with its own distinctive 'life.'

A conception of this kind, based on biological analogy, is seductive, but it seems to me to be a false analogy. At all events we have no definite proof that such a 'group mind' occurs, and the whole conception places it out of the realms of biology and anthropology and transfers it to that of psychology or of metaphysics, and there I must leave it.

To revert to the committee, or other group of men, it appears to me that the decision arrived at may simply be a conscious or unconscious common factor that emerges during discussion, or a factor which, though not common to every individual, is so to the majority. The action of the group ultimately is true to the particular type of the group, that is, the usual policy or the definite trend of practicality or idealism which characterizes that one group. In a sense one may say that the group has a 'mind' or a 'trend,' but not in the same manner that an animal has a 'mind' or a 'trend of development.' For I maintain that this 'mind' or 'trend' is latent in at least the majority of the individuals of the group and is released by discussion and by inhibition of other lines of thought.

SIMULTANEOUS CONTRAST UNDER VARIOUS CONDITIONS

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Some time ago I had the opportunity of making a series of researches into simultaneous contrast ("A Method for Measuring Simultaneous Contrast," this *Journal*, XVII, pt. 2, Oct. 1926). I assumed that the phenomenon, which is usually called irradiation must be looked upon as the resultant of two component factors, viz. physical aberration of light and simultaneous contrast. Or rather those sensations, which we perceive subjectively as 'irradiation phenomena,' are in reality caused by the aberration of light but are partly corrected by simultaneous contrast. Starting from the classic experiments of Plateau these were modified so that instead of pieces of white paper on a black background I made use of a pencil of light thrown on a dead-silver screen in a perfectly dark room. This pencil of light was produced by introducing a metal lamina above the large lens of a Zeiss epidiascope. In this lamina was a square aperture of 1 cm. through which, by alteration of focus, a clearly or vaguely defined square of white light could be projected upon a neutral dark background. When an image was projected on the screen it seemed to be measurably smaller than when thrown on a piece of sensitive paper. The vaguer the boundary, the more obvious was the difference of size. To study this phenomenon accurately I sought a method of measuring the 'objective' and 'subjective' sizes of the spot of light in rapid succession, and under the same circumstances. The measurement was effected as follows:

A. Objective size was determined by projecting the square spot of light upon a metal screen which could be moved by a screw into a horizontal direction. In the middle of the screen was a small hole. The examiner, standing behind the screen looked through this hole and moved the screen till he had reached the ends of the light area. The distance through which the screen had been moved could be read from a scale division.

B. Subjective size was determined by the projection of two squares of light in such a way that they touched in one angular point and that the diagonal of one square was the production of the other. One square was movable with respect to the other in the direction of the common diagonal. Starting from the position in which the squares touch at one angular point but were not in exact focus the observer obtained a hazy projection, the irradiation phenomenon appeared and the two lighted areas became confluent. A bridge of light was formed between the two lighted areas and the one sideline was no longer the production of the other. Next the situation of the one square with respect to the other was so changed that the observer, placed at a considerable distance from the screen, got the impression that the two sidelines of the squares were again each other's production. Sharp focus was once more secured and the two squares were found no longer to touch in one point; a dark zone appeared between them. The distance of the two nearest angular points of the squares could be taken as a measure of the irradiation and so, indirectly, of the simultaneous contrast.

By means of these methods, which are accurately described in my earlier paper, simultaneous contrast was measured.

The following values were found:

A. *Objective Values.* (Cross-section of illuminated area in mm.)

focused exactly				193
hazy adjustment (face of lens		1 cm. lower)		205
,,	,,	2	,,	220
,,	,,	3	,,	240

B. *Subjective Values.* (Cross-section of illuminated area in mm.)

focused exactly				191
hazy adjustment (face of lens		1 cm. lower)		196·5
,,	,,	2	,,	200·4
,,	,,	3	,,	207·1

The difference between objective and subjective values is a measure of simultaneous contrast. Thus we have:

C. *Simultaneous Contrast*

focused exactly				2
hazy adjustment (lens		1 cm. lower)		8·5
,,	,,	2	,,	19·6
,,	,,	3	,,	33

In the present paper I shall illustrate applications of my method in the determination of values of simultaneous contrast for light of various intensities and colours[1].

First 'objective values' were measured, *i.e.* the size of the spot of light with its aberration phenomena; then 'subjective values,' *i.e.* the size of the spot of light, as it is seen by an observer at a distance. The difference between these values, which are expressed in mm. was used as a measure of the simultaneous contrast.

The difference in intensity of light was obtained by moving in the epidiascope the diaphragm in such a way that the full quantity of light was diminished first to 50 per cent., then to 25 per cent. of the maximum. The following values were found:

Table I.

White.

	Objective				Subjective				Simultaneous contrast			
Percentage of light	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	193	205	220	240	191	196·5	200·4	207·1	2	8·5	19·6	33
50	192	202	214	228	191	197	202	206	1	5	12	22
25	192	198	207	219	191	195	200	202	1	3	7	17

To show the matter more clearly I have plotted curves of my main observations.

Curve 1. Objective and subjective values in 100 per cent. white light.

The rising line of both objective and subjective values shows an almost straight line. The highest value, which is obtained by maximum light and a removal of the lens 3 cm. lower is 240 mm. At the same position of the lens the subjective value is but 207·1, so that the simultaneous contrast in this case has an extremely high value.

Curve 2. Objective and subjective values in 50 per cent. white light.

It is remarkable that the two lines, which are also almost straight, make a much smaller angle with each other. This is still more markedly the case in Curve 3.

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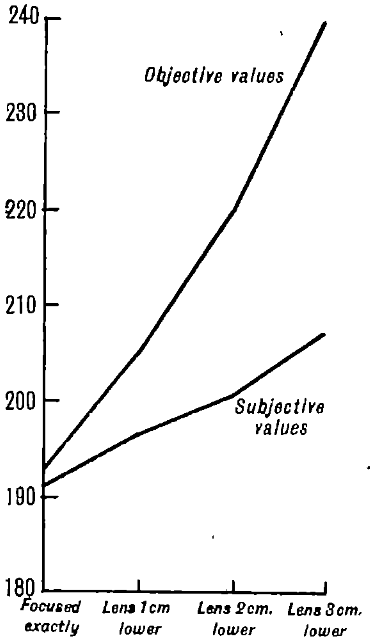
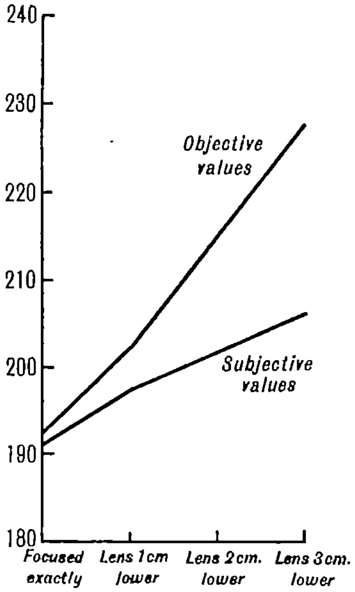
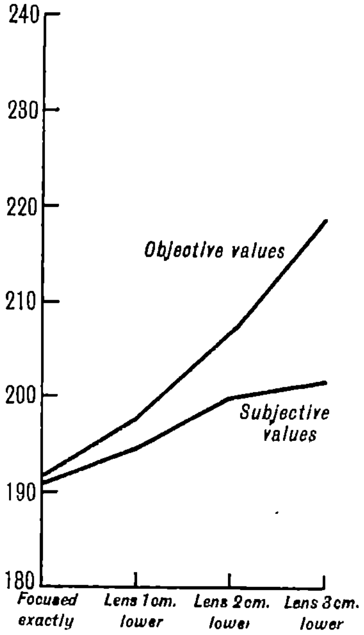
[1] In all observations recorded the distance of the observer from the objects observed was 5 m. The angular size of the observed object and the amount of illumination were the same as recorded in my earlier paper (this *Journal*, xvii, pt. 2).



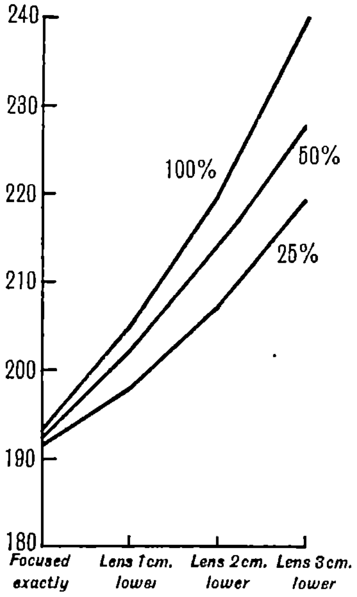
Curve 1. Objective and subjective values in 100 % white light.



Curve 2. Objective and subjective values in 50 % white light.



Curve 3. Objective and subjective values in 25 % white light.



Curve 4. Objective values in different percentages of white light.

Curve 3. Objective and subjective values in 25 per cent. white light, where the angle is about ⅓ of that indicated in Curve 1.

Curve 4. Objective values in different percentages of white light.

When we compare in Curve 4 the objective values in different percentages of white light, we see a large deviation of these lines, though the mutual difference is almost the same.

This is not the case in Curve 5.

Curve 5. Subjective values in different percentages of white light.

These differ too, but not so sharply as the objective values. The great difference of angle between the lines which represent objective and subjective values at 100 per cent., 50 per cent. and 25 per cent. of white light is mainly due to a difference in the objective values.

Curve 6. Simultaneous contrast in different percentages of white light.

Simultaneous contrast in different percentages of white light shows that with decrease of light simultaneous contrast also decreases definitely. All values given are averages of from two to five observations. The observations themselves are somewhat difficult, particularly those with hazy adjustment.

The same series of experiments was next made with red light obtained by filling the cooler of the epidiascope with a solution of eosine instead of water. This gives a highly saturated red. The following values were found:

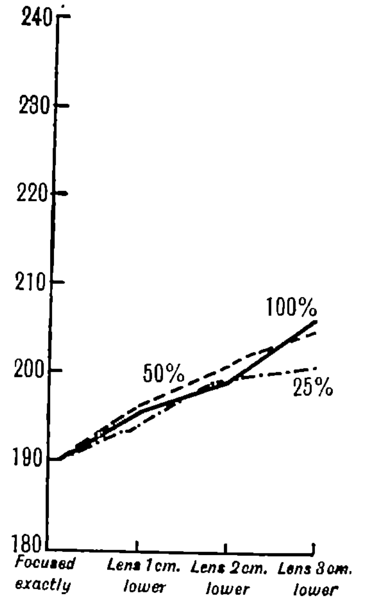
Table II.

RED.

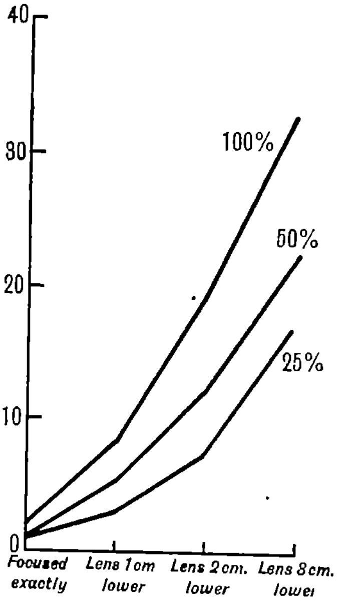
Percentage of light	Objective				Subjective				Simultaneous contrast			
	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	185	193	207	220	184	190	198	203	1	3	9	17
50	185	190	200	209	184	188	195	198	1	2	5	11
25	185	188	194	200	184	187	191	193	1	1	3	7

Curve 7. Objective and subjective values in 100 per cent. red light.

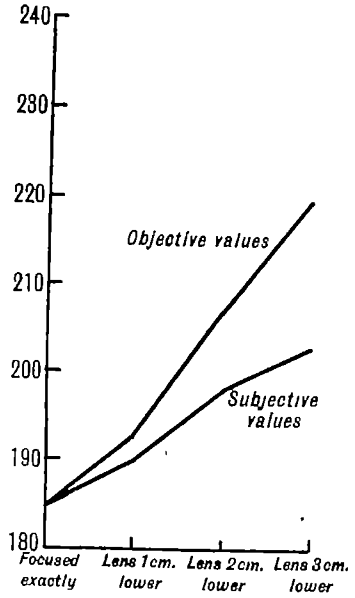
The lines, representing objective and subjective values are not straight. The angle, made by the lines indicating objective and subjective values in different percentages of light, is also different from that obtained with white light, being on the whole smaller.



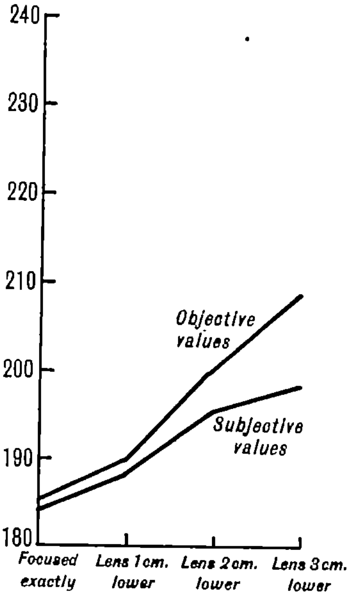
Curve 5. Subjective values in different percentages of white light.



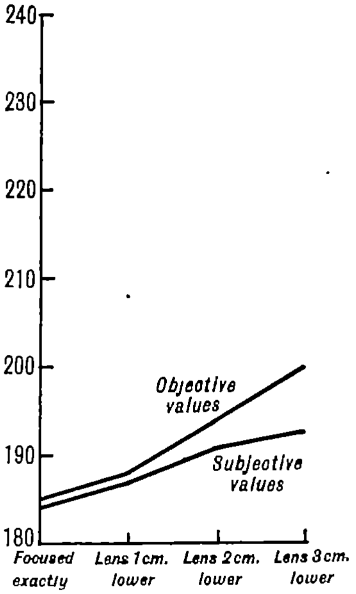
Curve 6. Simultaneous contrast in different percentages of white light.



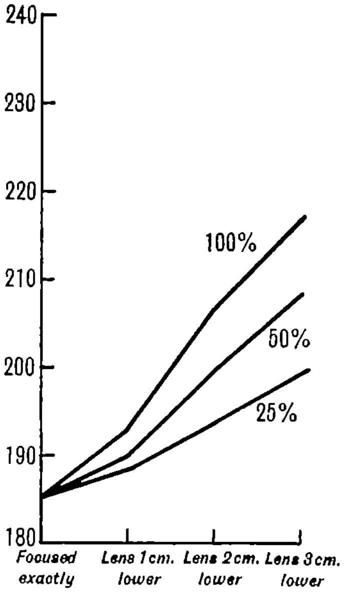
Curve 7. Objective and subjective values in 100 % red light.



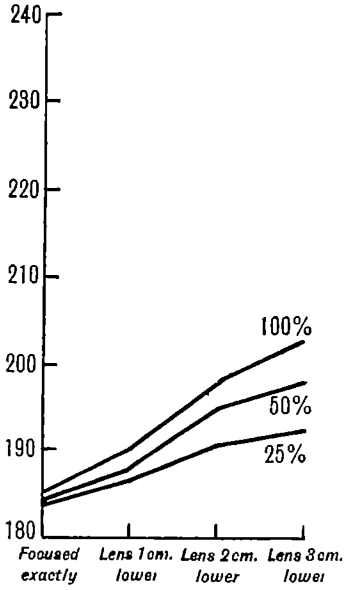
Curve 8. Objective and subjective values in 50 % red light.



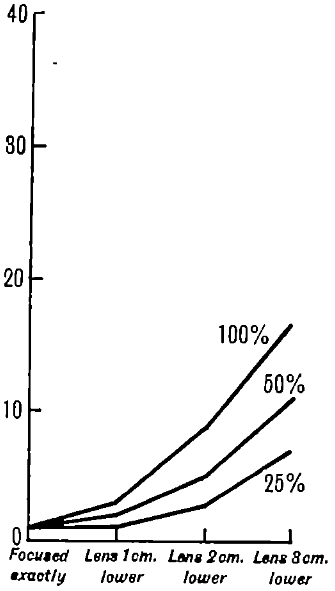
Curve 9. Objective and subjective values in 25 % red light.



Curve 10. Objective values in different percentages of red light.



Curve 11. Subjective values in different percentages of red light.



Curve 12. Simultaneous contrast in different percentages of red light.

Curve 8. Objective and subjective values in 50 per cent. red light.

Curve 9. Objective and subjective values in 25 per cent. red light.

Curve 10. Objective values in different percentages of red light.

This agrees well with Curve 4, *i.e.* that of the objective values in different percentages of white light; here, however, the values are all lower.

Curve 11. Subjective values in different percentages of red light.

This curve does not resemble Curve 5. In general the subjective values in red light differ much more than those in white light. Thus the difference of angles between the lines of the objective and subjective values is in red light not especially due to an alteration of the objective values.

Curve 12. Simultaneous contrast in different percentages of red light.

Next the same experiments were made with saturated blue light, obtained by filling the cooler of the epidiascope with a solution of indigo blue.

Table III.

BLUE.

	Objective				Subjective				Simultaneous contrast			
Percentage of light	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	189	198	213	230	188	195	203	212	1	3	10	18
50	188	196	206	218	187	193	201	205	1	3	5	13
26	187	194	198	210	186	191	194	202	1	3	4	8

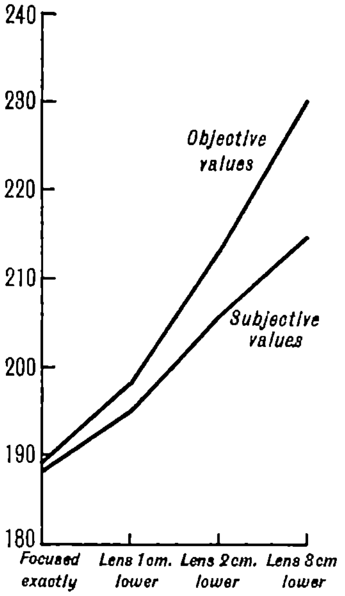
Curve 13. Objective and subjective values in 100 per cent. blue light.

This agrees with Curve 1. The values are now lower, but they are higher than those in red light, and the same holds good in all the following curves. Generally speaking all values for blue and green light are higher than the corresponding values for red light. This agrees with the fact that the blue side of the spectrum shows more aberration-phenomena than the red side.

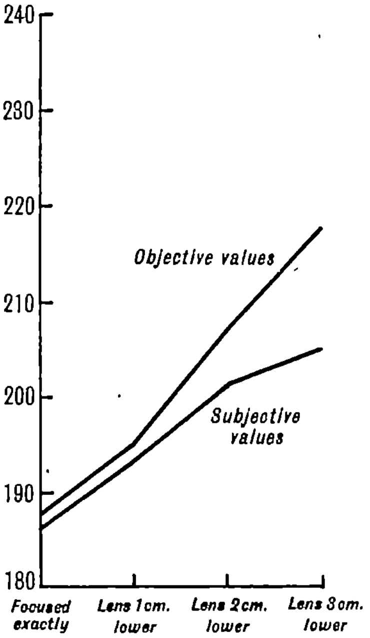
Curve 14. Objective and subjective values in 50 per cent. blue light.

Curve 15. Objective and subjective values in 25 per cent. blue light.

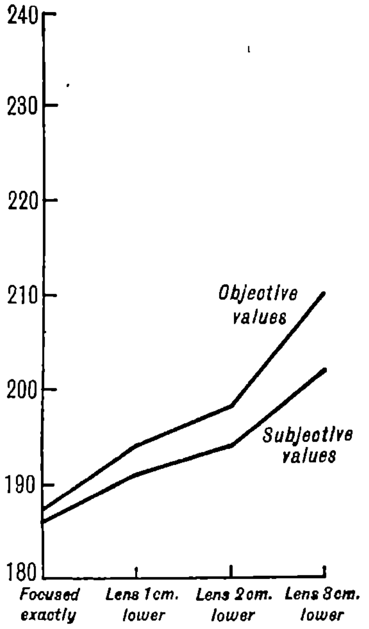
Curves 14 and 15 show a small angular difference between objective and subjective values.



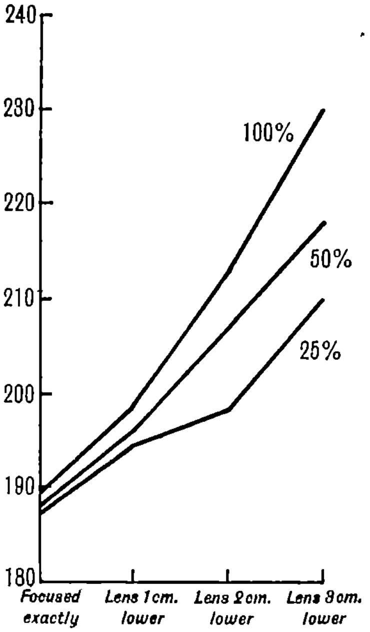
Curve 13. Objective and subjective values in 100 % blue light.



Curve 14. Objective and subjective values in 50 % blue light.



Curve 15. Objective and subjective values in 25 % blue light.



Curve 16. Objective values in different percentages of blue light.

Curve 16. Objective values in different percentages of blue light.

This resembles Curves 4 and 10, which represent the same values in white and red light.

Curve 17. Subjective values in different percentages of blue light.

Curve 18. Simultaneous contrast in different percentages of blue light.

We find that in 100 per cent., 50 per cent. and 25 per cent. of blue light, the simultaneous contrast at first increases equally.

When the hazy adjustment is increased through the lowering of the lens 1 cm. or more the relative values differ.

Finally the experiments were repeated with a highly saturated green light, obtained by filling the cooler with a solution of malachite green. The results were as follows:

Table IV.

GREEN.

	Objective				Subjective				Simultaneous contrast			
Percentage of light	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	187	196	215	229	186	193	206	210	1	3	9	19
50	187	195	207	218	186	193	201	205	1	2	6	13
25	186	193	197	211	185	191	192	203	1	2	5	8

Curve 19. Objective and subjective values in 100 per cent. green light.

Generally speaking the values found with green light differ but slightly from those with blue light. Determination of the subjective values in green light was the most difficult of all.

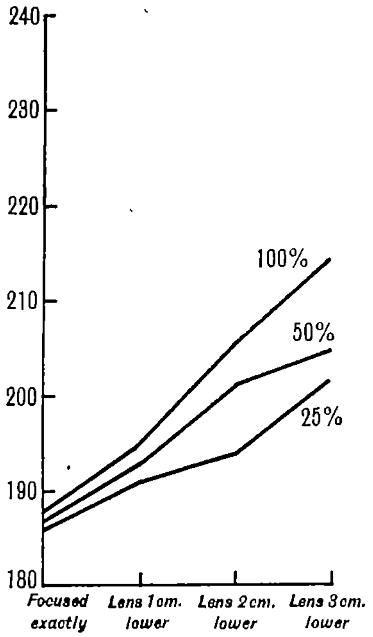
Curve 20. Objective and subjective values in 50 per cent. green light.

Curve 21. Objective and subjective values in 25 per cent. green light.

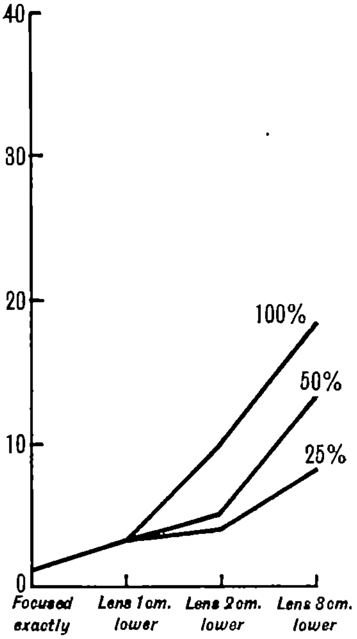
This gives evidence in both lines of a remarkably small increase of the values, when the hazy adjustment is changed by lowering the lens from 1 cm. to 2 cm. But when the lens is lowered to 3 cm. there is in both cases an increase of the values.

Curve 22. Objective values in different percentages of green light.

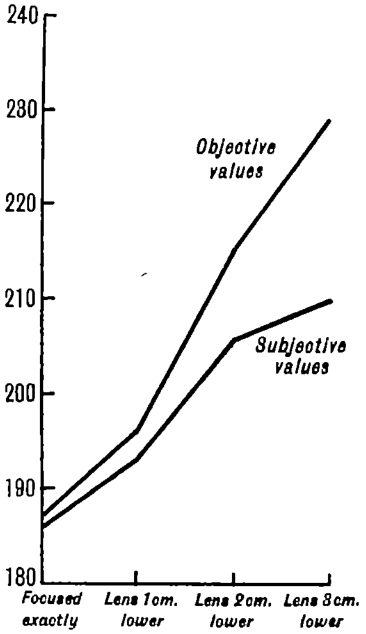
This is very similar to the corresponding curves for white, red and blue light.



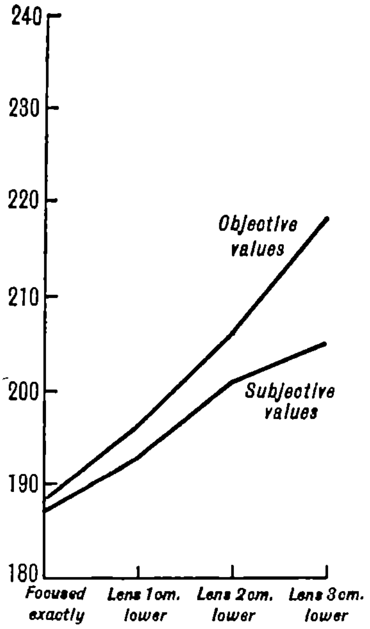
Curve 17. Subjective values in different percentages of blue light.



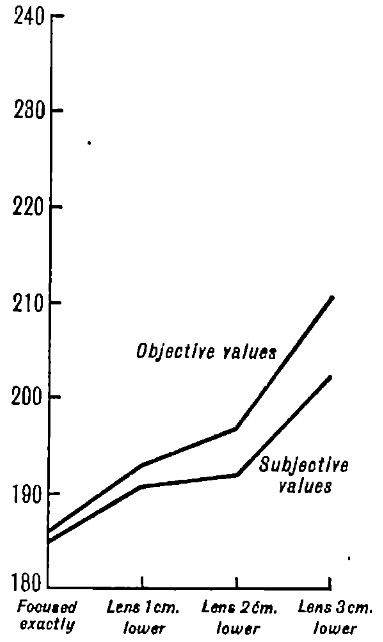
Curve 18. Simultaneous contrast in different percentages of blue light.



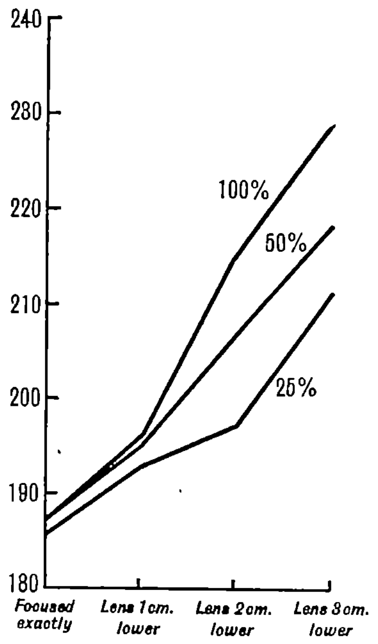
Curve 19. Objective and subjective values in 100 % green light.



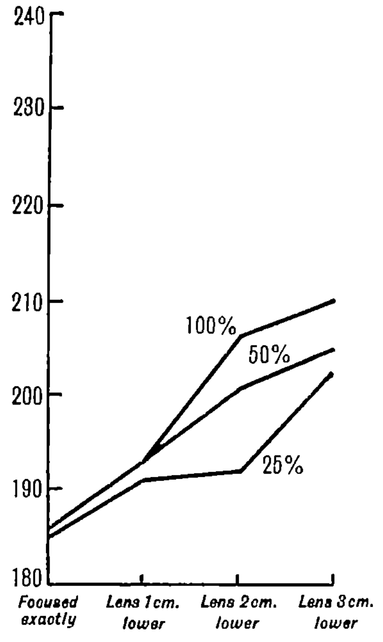
Curve 20. Objective and subjective values in 50 % green light.



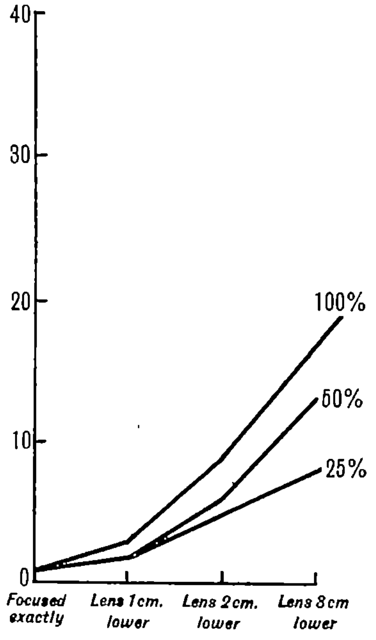
Curve 21. Objective and subjective values in 25 % green light.



Curve 22. Objective values in different percentages of green light.



Curve 23. Subjective values in different percentages of green light.



Curve 24. Simultaneous contrast in different percentages of green light.

Curve 23. Subjective values in different percentages of green light.

This shows that in the beginning of the hazy adjustment (lens 1 cm. lower) there is no difference between 100 per cent and 50 per cent. of light, when the hazy adjustment increases the values differ. When there is only 25 per cent. of light the whole·curve shows lower values.

Curve 24. Simultaneous contrast in different percentages of green light.

Simultaneous contrast in different percentages of green light shows the same values in 50 per cent. and 25 per cent. of light, in hazy adjustment, when the lens is lowered 1 cm.

After this the experiments were repeated once more but now using less saturated light. This effect was reached by diluting the liquid in the cooler of the epidiascope. The result of this dilution was judged by comparing with a colour-skala. A colour was chosen, which is composed of 50 per cent. white and 50 per cent. red light (semi-red). I got the following results:.

Table V.

SEMI-RED.

	Objective				Subjective				Simultaneous contrast			
Percentage of light	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	190	198	214	232	189	194	200	205	1	4	14	27
50	189	197	208	220	188	193	199	203	1	4	9	17
25	189	194	201	209	188	192	196	198	1	2	5	11

Generally speaking it may be said that the values of these experiments with semi-red light lie between those for white and red. On analogy of the preceding the following curves were made:

Curve 25. Objective and subjective values in 100 per cent. semi-red light.

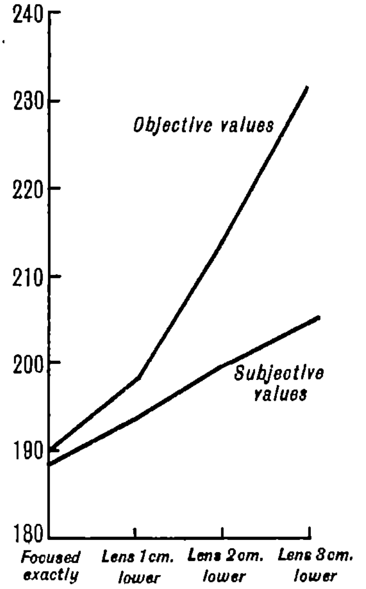
Curve 26. Objective and subjective values in 50 per cent. semi-red light.

Curve 27. Objective and subjective values in 25 per cent. semi-red light.

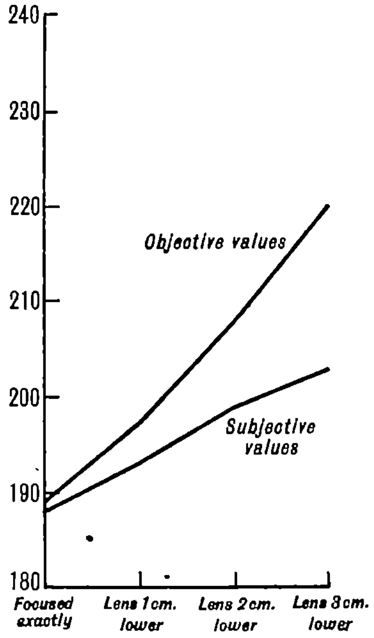
Curve 28. Objective values in different percentages of semi-red light.

Curve 29. Subjective values in different percentages of semi-red light.

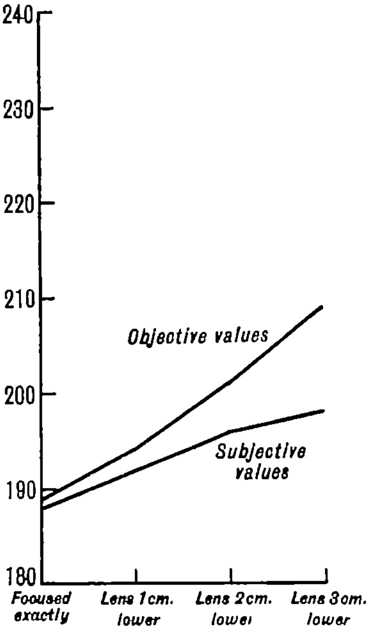
Curve 30. Simultaneous contrast in different percentages of semi-red light.



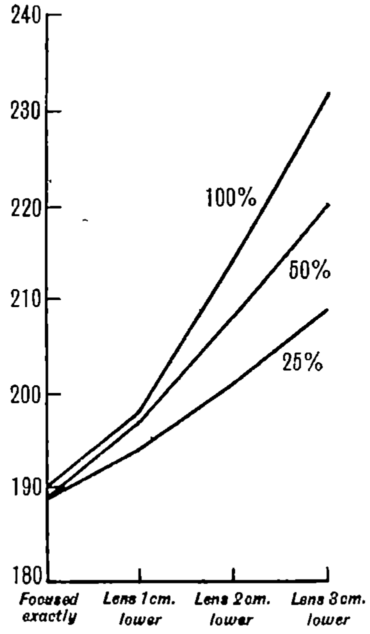
Curve 25. Objective and subjective values in 100 % semi-red light.



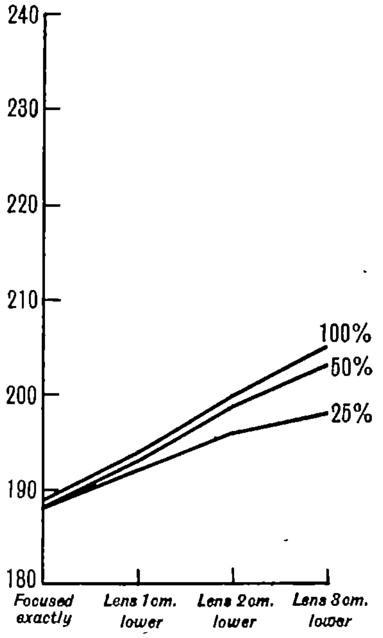
Curve 26. Objective and subjective values in 50 % semi-red light.



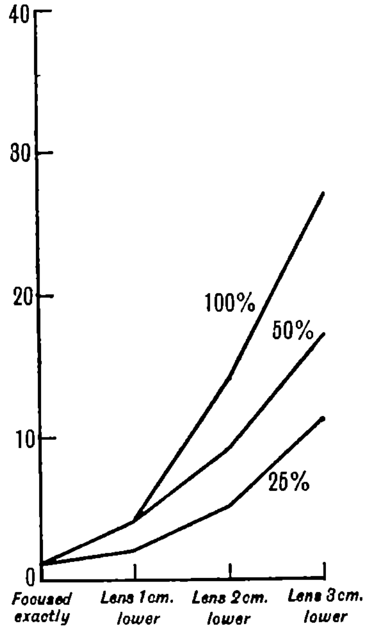
Curve 27. Objective and subjective values in 25 % semi red light.



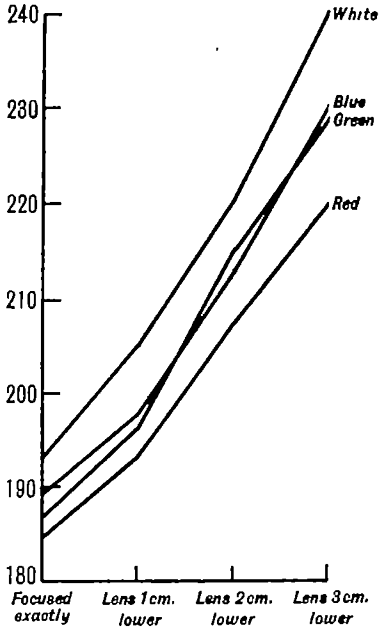
Curve 28. Objective values in different percentages of semi-red light.



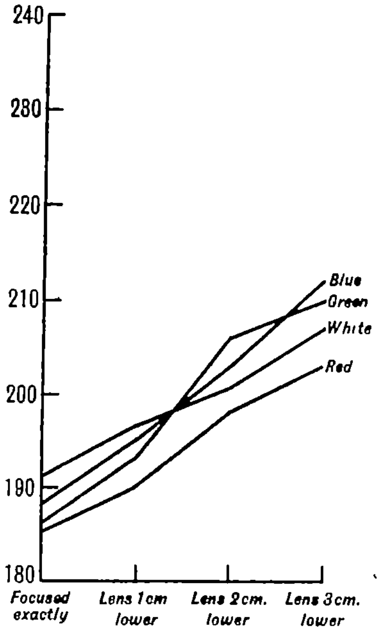
Curve 29. Subjective values in different percentages of semi-red light.



Curve 30. Simultaneous contrast in different percentages of semi-red light.



Curve 31. Objective values in light of various shades.



Curve 32. Subjective values in light of various shades.

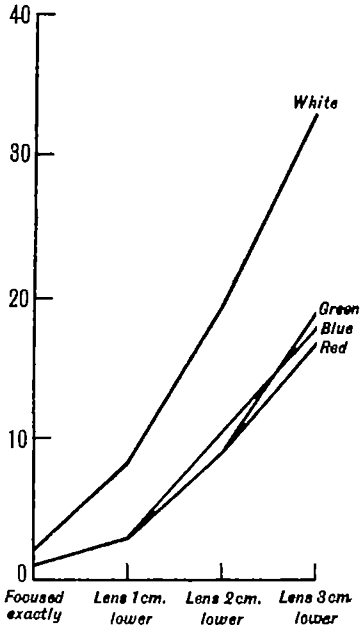
The values lie everywhere between white and red and need no particular description. The same experiments were made with blue light, which was diminished in saturation in such a way that we may consider it to be composed of 50 per cent. of white and 50 per cent. of blue light.

Tables only will be given of experimental results in lightly saturated blue and green.

Table VI.

SEMI-BLUE.

	Objective				Subjective				Simultaneous contrast			
Percentage of light	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	192	202	217	237	191	197	206	216	1	5	11	21
50	190	199	211	225	189	195	202	207	1	4	9	18
25	190	196	205	216	189	192	200	203	1	4	5	13



Curve 33. Simultaneous contrast in light of various shades.

Table VII.

SEMI-GREEN.

Percentage of light	Objective				Subjective				Simultaneous contrast			
	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower	Focused exactly	Lens 1 cm. lower	Lens 2 cm. lower	Lens 3 cm. lower
100	191	200	215	234	190	196	206	214	1	4	9	20
50	190	198	210	221	189	195	204	207	1	3	6	14
25	189	196	203	213	188	194	198	202	1	2	5	11

Finally the following curves are worth some attention:

Curve 31. Objective values in light of various shades.

Objective values in light of various shades shows clearly that the irradiation phenomena stand out best in white light, least in red light, and are intermediate in green and blue.

Curve 32. Subjective values in light of various shades.

Subjective values in light of various shades shows that in subjective values the above rule does not exist. But clearly the values for red are the least.

Curve 33. Simultaneous contrast in light of various shades.

Simultaneous contrast in light of various shades shows that in coloured light the values of simultaneous contrast differ less and that they are much smaller than in white light. Here too red is the lowest.

(*Manuscript received* 18 *April* 1927.)

AN EXPERIMENTAL INVESTIGATION INTO TRANSFER OF TRAINING IN SKILLED PERFORMANCES

By J. N. LANGDON and EDNA M. YATES.

(*An investigation carried out for the Industrial Fatigue Research Board in the Psychological Laboratory, University of Manchester.*)

(1) *Statement of Problem* (pp. 422–423).
    (*a*) *Sources of experimental error* (p. 423).
(2) *Experiment on Transfer of Training in Manual Dexterity* (pp. 423–432).
    (*a*) *Constancy of conditions* (pp. 427–428).
    (*b*) *Treatment of results* (pp. 428–431).
    (*c*) *Reliability of tests* (p. 431).
    (*d*) *Conclusions* (pp. 431–432).
(3) *Experiment on Transfer of Training in Visual Discrimination* (pp. 432–436).
    (*a*) *Treatment of results* (pp. 435–436).
    (*b*) *Conclusion* (p. 436).
(4) *Summary of conclusions from both experiments* (pp. 436–437).

(1) STATEMENT OF PROBLEM.

The doctrine of formal discipline states that the effect of training in any specific form of mental activity may be transferred to any other activity of the same form, although dealing with different material.

Since the early experiment of William James[1], the problem of transfer of training has been the subject of much experimental work. The results obtained have led to very conflicting conclusions.

The essential order of such experiments may be summarized thus: (*a*) test, (*b*) training in similar but not identical material, then (*c*) further test.

Unfortunately many of the experiments have not been performed with a sufficient number of subjects to allow of any statistical treatment. Many of the experimental results have been obtained from groups containing as few as six subjects. In such cases no reliability can be attached to any representative measure, and the significance of any differences between trained and untrained subjects can remain only a matter of opinion.

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[1] *Principles of Psychology*, I, 666.

(a) *Sources of experimental error.*

In dealing with previous work on transfer of training in memory, Dr W. G. Sleight criticized the absence of control-subjects to determine the practice effect of the tests themselves, and the lack of evidence that the first and second transfer tests were of equal difficulty[1].

A further objection has been suggested by Prof. E. L. Thorndike[2] to the effect that material and conditions are very artificial, and that subjects would usually not take more than a passing interest in their performance.

Prof. T. H. Pear[3] further emphasizes the necessity for constant motivation in each of the transfer tests. He points out that there is usually no assurance, beyond the goodwill of the subject, that he has the same incentive to satisfactory performance in the first and in the final transfer test. Finally, in a great many investigations, the training has not been intensive, consisting merely of a brief daily practice continuing for various lengths of time.

(2) EXPERIMENT ON TRANSFER OF TRAINING IN MANUAL DEXTERITY.

The following experiment was planned under the direction of Prof. Pear with the view of investigating transfer in muscular performances. We are also indebted for advice and criticism to Mr S. Wyatt, and, particularly with regard to the treatment of results, to Mr H. E. O. James.

The details of the experiment were planned with constant reference to the criticisms mentioned above.

The operation for the intensive training of the subjects is a modification of one used in the driving-chain industry[4]. In an investigation carried out in another connection by Mr Wyatt[5], it was discovered that this operation gives a reliable practice curve. We are indebted to him for the laboratory adaptation of the apparatus. The essential features of the process are these:

The subject is seated in front of a table on which is fixed a circular turn-table which carries spindles on which links are placed. As each spindle comes into position before the subject, he removes it from the turn-table, dropping the link into a box at his right hand. At the same time he takes another link from a box at his left hand, places it on the spindle, which he re-inserts in the turn-table, and rotates the table so as to bring the next spindle into position for this operation to be repeated—see Fig. 1 (reproduced by kind permission of the Manchester Literary and Philosophical Society).

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[1] This *Journal*, 1911.

[2] "Mental Discipline in High School Studies," *Journal of Ed. Psychology*, xv, 1924.

[3] "Recent Studies of Skilled Performances with Reference to Transfer of Training," *Nature*, 18 June, 1927.

[4] The experimenters would like to thank Messrs Hans Renold, Ltd., who kindly supplied a quantity of links for use with the apparatus.

[5] "Machine Speeds and Output," *Journ. Nat. Inst. Ind. Psy.* III, 8, 1927.

The score, recorded automatically by a Veeder counter operated by the rotation of the turn-table, is the number of links dealt with in a period of 10 minutes' work. A period of 10 minutes' rest alternated with each period of. 10 minutes' work, and the subject worked for eight such periods, both morning and afternoon. A typical practice curve appears in Fig. 2, showing for both morning and afternoon the average number of links dealt with during each 10 minute period. Although it is evident from this that the full effect of practice is not yet attained, improvement in general was more than 50 per cent. of the initial score.

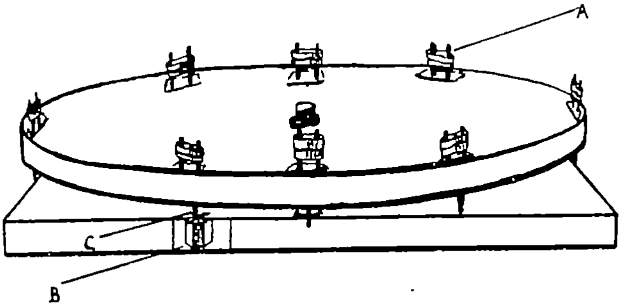

Fig. 1. Diagram of Apparatus.
A—Link and spindle. B—Veeder counter. C—Spike engaging arm of counter.

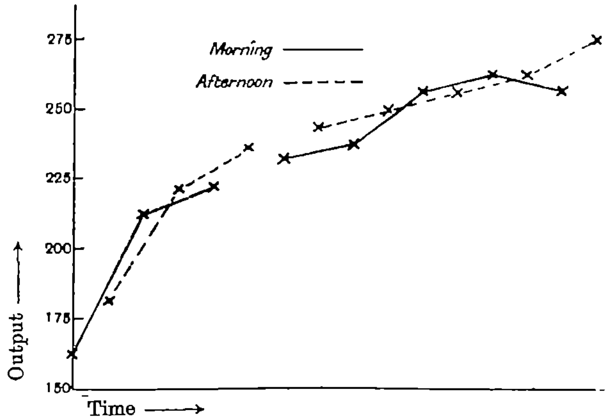

Fig. 2. Practice curve in operation with links.

Altogether 32 subjects were trained in this manner, each being employed for 2 weeks. These constituted the trained group.

Before training was commenced, the performance of each subject was measured in the various tests designed to show the presence or absence of transfer. These tests were selected in accordance with an observational analysis of the operation with the links and spindles described above. It will be seen that many of them are simple tests of manual dexterity—they are as follows:

(1) *Match insertion.* The subject sat in front of a small board pierced with holes, in each of which a match could be inserted. He took matches one by one from a box placed in a constant position, and endeavoured to insert as many matches as possible in the time allowed. The score was the number of matches inserted in 2 minutes. The test was performed with both right and left hands separately.

(2) *Placing matches in a box.* Thirty matches, separated from each other by a distance of roughly 1 cm., were laid in a straight line. The subject held a match-box in his left hand, and with his right hand picked the matches one by one from the table. The matches had to be placed neatly in the box with the heads all one way. When this had been done, the subject fixed the cover on the box and closed it. This precaution ensured that the subject had complied with the instructions as to neatness. The score was the time taken. The experiment was then repeated with the left hand.

(3) *Placing rings on a rod.* The subject took curtain rings, one at a time, from a bowl placed in a constant position within easy reach of the hand he was employing. He slipped them as quickly as possible over a retort stand placed in front of him. The experiment was performed with both right and left hands separately. The score was the number of rings dealt with in 2 minutes.

(4) *Threading links.* The subject held a large needle threaded with twine. At his free hand was placed a pile of links—those used in the chain-assembling operation. He picked up the links one at a time and threaded them over the needle. This test also was performed with both right and left hands separately. The score was the number of links threaded in 2 minutes.

(5) *Arm movement.* The apparatus consists of a cardboard semicircle of 2 feet radius. Along the circumference of this is fixed a graduated scale. At the centre is fixed a movable pointer which rotates on small wheels. At the head of the pointer is an attachment into which the subject can insert his fingers. The subject stands with his hip against the centre of the semicircle. He rests his hand on the broad head of the

pointer, with his fingers in the place provided. With eyes closed, he is required to make a rotatory movement of the arm. He is then instructed to try to repeat the movement of exactly the same extent as before. He makes the movement in this manner altogether ten times. The score in this test was the coefficient of variation, *i.e.* the mean deviation from the mean expressed as a percentage of the mean.

(6) *Moede's impulse meter*[1]. The subject holds a hammer in his right hand and is instructed to strike a specially devised anvil. The force of the blow is measured on the scale. He is then told to strike the anvil again with just the same force as before. He repeats this ten times in all. Here again the score was the coefficient of variation.

(7) *Steadiness, Test* 1. The apparatus employed was that described in Whipple's *Manual of Mental and Physical Tests*[2], I, 157. It consists of a metal plate set at an angle of 45°, pierced by holes of diameter 32, 20, 16, 13, 11, 10, 9, 8, 7 sixty-fourths of an inch. This plate is in electric circuit with a buzzer and a metal stylus held by the subject. When contact between plate and stylus occurs, the buzzer is thereby sounded. The method was as follows. The subject was instructed to hold the stylus in each of the holes in turn, from the largest to the smallest. The stylus was held in each hole for 15 seconds, with a preliminary 3 seconds before completing the electric circuit, in order to give the subject time for adaptation. At the first contact the experiment was stopped and commenced again from the first hole. This was repeated seven times. The score in each of the seven trials was the total time elapsing before contact, and the final score was the median of these seven.

✓

Steadiness, Test 2. The apparatus is described in Whipple, I, 152. It is the usual tracing board, with a slit bounded by converging metal strips. Along one edge is mounted a millimetre scale. The subject was instructed to draw the stylus along the slit without making contact with the sides. At the first contact the subject stopped and repeated the process. This was repeated eleven times. The score in each case was the distance traversed before contact, and the final score was the median of these eleven.

Mental arithmetic. The material consisted of pages of sums of the following type: 3 × 9 = 30, 27, 12, 15. The subject was instructed to underline the correct answer. The score was the number of answers correctly marked in 4 minutes.

Cancellation. The material is described in Whipple, I, 126. The subject was instructed to cross out every q, r, s and t on the printed form. The time taken was recorded with a stop-watch. The score was calculated from the formula given by Whipple[1].

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[1] Illustration given in the catalogue of E. Zimmermann, Leipzig.

[2] Warwick and York, Baltimore, 1924.

These last two tests have no obvious connection with the process of chain-assembling. It was assumed that if the presence of transfer were demonstrated in all the tests, in these last, to as great a degree as in the tests of manual dexterity, there would be reason to suppose that transfer had its origin in the cultivation of a general mental attitude towards testing, in habituation to environment, or in some general factor rather than in specific practice.

This series of tests was given on three occasions. These were before training had commenced, at the end of the first week, and at the end of the fortnight. These transfer tests may be called 1, 2 and 3.

The same tests were given in the same order and at the same intervals of time to 28 subjects who received no training. These constituted the control group.

(a) *Constancy of conditions.*

We may now consider the attempts which were made to meet the criticisms which have been directed towards this type of experiment.

(1) *Adequate motivation.* The subjects were all boys between the ages of 15 and 18, whose services were obtained through the agency of the Juvenile Employment Bureau[2]. They were paid a minimum wage of 12*s.* per week, and in addition to this received a daily bonus depending entirely on the degree of improvement effected day by day. It was possible for a good worker to earn on an average two to three shillings in addition in this manner each day. By this means an adequate incentive was assured.

On the days on which transfer tests were given, the subjects' bonus was made up on the basis of improvement in these. Thus there was the same incentive as in training and the motivation was constant from test to test.

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[1] $$E=\frac{C-W}{C+O}\times\frac{100}{T},$$

where E = index of efficiency;
C = number of letters crossed;
W = number of letters wrongly crossed;
O = number of letters erroneously omitted;
T = time in seconds.

The subject was informed of the method of scoring and was thus aware that both speed and accuracy were essential.

[2] We should like to take this opportunity of thanking Mr Greenwell and his staff for their kind assistance.

(2) *Comparability of trained and control groups.* Subjects for control and trained groups were chosen from the same age and class of boy at the Juvenile Employment Bureau. The control group was paid on the same system as the trained group. The individual amounts earned on the occasion of the first giving of the tests were based upon the performance of individual subjects, and on the two second occasions on improvement on the previous performance in each of the tests.

Thus it seems legitimate to assume that in both trained and control groups the financial incentive was identical.

(3) *Constant motivation.* Attention has already been drawn to the fact that in the usual laboratory experiment, there is no assurance that motivation is constant from test to test. Subjects are usually undergraduates who volunteer, or who are persuaded by friends to give their services to the laboratory. With subjects of this type it is exceedingly probable that the attitude to the test will vary considerably on different occasions, and that what may be called the 'worth-whileness' of the performance will vary with changes in the mood of the subject.

In this experiment these factors were eliminated as far as could be ascertained. The desire to earn as much as possible was a constant motive apparently prominent in consciousness. Information obtained by questioning the subjects confirms this statement. Since the influence of the experimenter in testing can seldom be eliminated, each subject worked under the same experimenter for each of the three successive transfer tests[1].

(4) *Equal difficulty of successive transfer tests.* In this experiment this objection is met by the fact that the tests given on the three occasions were identical. Naturally there is a training effect due to practice in the test itself, but this can be assumed to be the same for both control and trained groups.

(5) *Working conditions.* The subjects were intensively trained for a fortnight, with the omission of days on which 'transfer tests' were given. They were all subject for 3 hours, both morning and afternoon, to the laboratory conditions and discipline.

(6) *Number of subjects.* The trained group contained 32, and the control group 28 subjects. Statistical treatment of the results is therefore possible.

(*b*) *Treatment of results.*

The method adopted is that of the comparison between means.

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[1] Cf. Burnett and Pear, "Motives in Acquiring Skill," This *Journal*, 1925.

If the difference between the means of two groups of measurements is at least three times as large as the standard deviation of this difference, statistical convention states that the difference may be considered to be significant and not explicable by chance factors. The mean difference between the scores in transfer tests 1 and 2, and in 1 and 3, was calculated together with the standard deviation of this difference, for both trained and control groups. These calculations are shown in the first four columns of the accompanying table. A negative sign indicates a difference in favour of the control group.

The significance or non-significance of the difference is shown in each case. The final problem is to show whether there is any significant difference between each of the mean differences appearing in each line of columns I and III and II and IV in the table. This is shown in columns V and VI. Here again the same method is employed. To take a concrete example from the table: The second line contains the results of the test Match insertion, L. hand. In the second column we find that the mean difference between first and third performance of the trained group in this test is 6·81, with a standard deviation of 0·85. In the fourth column the mean difference between the first and third performance of the control group in the test is shown to be 4·93 with a standard deviation of 0·82. In the sixth column the difference between these mean differences is shown to be 1·88 with a standard deviation of 1·18. The standard deviation in the case of columns V and VI is the square root of the sum of the squares of the standard deviations from which it is derived.

In the example it is seen that the mean difference is not greater than three times its standard deviation and is therefore not significant. That is to say, there is no significant difference between the trained and the control groups with regard to the improvement effected between the first and the third giving of this transfer test.

The fifth column shows similar results relating to the first and second performances in the tests. It will be seen that in column VI there is no instance of significance. This may be interpreted that there is no instance in which the difference between trained and untrained subjects can be said with certainty to be not due to sampling errors. There is one significant case in column V relating to the difference between tests 1 and 2 in Match insertion, L. hand, but even in this the significance is lost by the time of the third and last test.

The results may be expressed in a more positive form. The appearance of significant differences in the first four columns and the entire absence of these in column VI may be interpreted that in such cases the brief

Table I

	I *A.* Trained	II	III *B.* Control	IV	V *C.* $A \sim B$	VI
	1–2	1–3	1–2	1–3	1–2	1–3
Match insertion, R. hand	M=3·03 σ_M=1·27 Not significant	M=4·72 σ_M=0·79 *Significant*	M=2·27 σ_M=0·79 Barely significant	M=4·23 σ_M=0·82 *Significant*	D=+0·76 σ_D= 1·49 Not significant	D=+0·49 σ_D= 1·14 Not significant
Match insertion, L. hand	M=6·35 σ_M=0·86 *Significant*	M=6·81 σ_M=0·85 *Significant*	M=2·90 σ_M=0·74 *Significant*	M=4·93 σ_M=0·82 *Significant*	D=+3·45 σ_D= 1·14 *Significant*	D=+1·88 σ_D= 1·18 Not significant
Match-box, R. hand	M=5·66 σ_M=1·39 *Significant*	M=4·94 σ_M=1·43 *Significant*	M=1·90 σ_M=1·35 Not significant	M=2·10 σ_M=1·84 Not significant	D=−3·76 σ_D= 1·49 Not significant	D=+2·84 σ_D= 2·33 Not significant
Match-box, L. hand	M=1·72 σ_M=1·22 Not significant	M=4·44 σ_M=1·11 *Significant*	M=2·03 σ_M=1·61 Not significant	M=2·87 σ_M=1·86 Not significant	D=−0·31 σ_D= 2·02 Not significant	D=1·57 σ_D=2·16 Not significant
Rings, R. hand	M=4·34 σ_M=0·96 *Significant*	M=6·09 σ_M=1·06 *Significant*	M=3·28 σ_M=1·03 *Significant*	M=5·45 σ_M=1·01 *Significant*	D=+1·06 σ_D= 1·41 Not significant	D=+0·64 σ_D= 1·46 Not significant
Rings, L. hand	M=3·66 σ_M=0·69 *Significant*	M=6·19 σ_M=1·34 *Significant*	M=4·18 σ_M=1·16 *Significant*	M=5·57 σ_M=1·35 *Significant*	D=−0·52 σ_D= 1·35 Not significant	D=+0·62 σ_D= 1·90 Not significant
Threading links, R. hand 15 sec. only	M=5·0 σ_M=1·68 *Significant*	M=5·80 σ_M=1·88 *Significant*	M=1·50 σ_M=1·52 Not significant	M=3·67 σ_M=1·19 *Significant*	D=+3·50 σ_D= 2·27 Not significant	D=+2·13 σ_D= 2·23 Not significant
Threading links, L. hand 15 sec. only	M=5·07 σ_M=1·69 *Significant*	M=4·27 σ_M=1·55 Not significant	M=1·83 σ_M=1·42 Not significant	M=5·00 σ_M=1·55 *Significant*	D=+3·24 σ_D= 2·21 Not significant	D=−0·73 σ_D= 2·19 Not significant
Arm movement	M=−0·93 σ_M= 0·88 *Significant*	M=−0·63 σ_M= 0·94 *Significant*	M=−0·32 σ_M= 0·71 *Significant*	M=−0·51 σ_M= 0·77 *Significant*	D=−0·55 σ_D= 1·14 Not significant	D=−0·12 σ_D= 1·21 Not significant
Impulse meter	M=1·67 σ_M=1·49 Not significant	M=1·91 σ_M=1·50 Not significant	M=4·17 σ_M=1·78 Not significant	M=2·88 σ_M=1·39 Not significant	D=−2·50 σ_D= 2·32 Not significant	D=−0·97 σ_D= 2·04 Not significant
Steadiness (static)	M=2·06 σ_M=1·93 Not significant	M=3·28 σ_M=2·42 Not significant	M=4·45 σ_M=2·59 Not significant	M=4·52 σ_M=0·83 *Significant*	D=−2·39 σ_D= 3·23 Not significant	D=−1·24 σ_D= 2·56 Not significant
Steadiness (dynamic)	M=8·06 σ_M=7·08 Not significant	M=11·56 σ_M= 8·29 Not significant	M=6·27 σ_M=7·14 Not significant	M=8·23 σ_M=9·27 *Significant*	D=+ 1·79 σ_D= 10·06 Not significant	D=+ 3·33 σ_D= 12·43 Not significant
Mental arithmetic	M=12·34 σ_M= 1·75 *Significant*	M=16·93 σ_M= 1·79 *Significant*	M=10·03 σ_M= 1·39 *Significant*	M=14·90 σ_M= 1·58 *Significant*	D=+2·31 σ_D= 2·23 Not significant	D=+3·03 σ_D= 2·39 Not significant
Cancellation	M=52·44 σ_M= 6·87	M=78·45 σ_M= 8·64	M=91·77 σ_M= 5·87	M=91·77 σ_M= 9·08	D=−1·03 σ_D= 9·04	D=−13·32 σ_D= 12·63

practice due to each test itself is more effective in producing improvement than is the intensive training in an analogous performance.

(c) *Reliability of tests.*

There remains for discussion the problem of reliability of the tests. Reliability of a test is usually measured by the size and significance of the correlation between successive applications of the test to the same subjects. Here the following observations should be noted:

(1) If the ability tested is modified by training, then poor correlation between successive applications of the test to the same subjects may reasonably be ascribed to the fact (supported by all that we know of learning in general) that some subjects learn more rapidly than others.

(2) The operation of this factor will be magnified in the initial stages of practice, because it is here that learning is most rapid.

(3) Some subjects did show appreciable improvement as the result of the small amount of practice given by the tests.

These observations may be illustrated by a table extracted from the article on "Motor Capacity" by B. Muscio[1]. He calculated the correlation between successive applications of tests at two stages: firstly, when practice was low, and secondly, when practice was high. The results are given below:

Tests	Initial reliability coefficients	Final reliability coefficients
Tapping	0·86	0·92
Aiming	0·70	0·82
Match-stick insertion	0·56	0·88

It will be noted that in each case the 'initial' is smaller than the 'final' coefficient.

In the present investigation, the subjects were tested on only three occasions. Consequently it is very unlikely that maximum practice was attained, and therefore the foregoing observations apply. That is to say, the correlations set out in the following table are probably, for the reasons given, greatly reduced below their true values. It will be seen however, that they are sometimes high, that they are always positive and rarely non-significant. In consequence reliability was considered adequate.

(d) *Conclusions.*

In no case was the difference between trained and control groups, with regard to improvement in the third over the first application of a test, of such a size as to exclude the possibility of its being due to chance factors. In some cases the brief practice, afforded by the test itself, is definitely shown to be more effective than the intensive training in an analogous performance.

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[1] "Motor Capacity, with Special Reference to Vocational Guidance,' This *Journal*, XIII, 1922.

Table II. *Correlations between successive applications of Transfer Tests*[1].

	1–2	2–3	1–3	Mean correlation
Match insertion, R. hand	0·7363 ±0·0589	0·7557 ±0·0553	0·7460 ±0 0572	0·7460
Match insertion, L. hand	0·6081 ±0·0813	0·5881 ±0·0844	0 5479 ±0·0924	0·5814
Match-box, R. hand	0·6577 ±0·0744	0·7557 ±0·0563	0·4669 ±0·1026	0·6268
Match-box, L. hand	0·6577 ±0 0731	0·6478 ±0·0748	0·5277 ±0·0930	0·6111
Rings, R. hand	0·5580 ±0·0903	0·6577 ±0·0719	0·7069 ±0·0656	0·6409
Rings, L. hand	0·4363 ±0·1080	0·8799 ±0·0301	0·3645 ±0·1157	0·5602
Threading links, R. hand	0·8610 ±0·0527	0·4465 ±0·1632	0·5381 ±0·1334	0·6319
Threading links, L. hand	0·7847 ±0·0783	0·6180 ±0·0479	0·7654 ±0·0844	0·7227
Arm movement	0·4973 ±0·0970	0·6280 ±0·0781	0·1674 ±0·1253	0·4309
Impulse meter	0·4669 ±0·1043	0·3748 ±0·1148	0·3850 ±0·1137	0·4089
Steadiness (static)	0·7654 ±0·0534	0·8135 ±0·0436	0·5881 ±0·0844	0·7223
Steadiness (dynamic)	0·5981 ±0·0828	0·4158 ±0·1067	0·3748 ±0·1107	0·4629
Mental arithmetic	0·8230 ±0·0416	0·8230 ±0·0416	0·8230 ±0·0416	0·8230
Cancellation	0·7654 ±0·0553	0·6580 ±0·0919	0·3645 ±0·1157	·0·5626

The results of the experiment support the view that training in manual dexterity is specific rather than general.

(3) EXPERIMENT ON TRANSFER OF TRAINING IN VISUAL DISCRIMINATION.

The problem in this experiment may be stated as follows:

Is the practice acquired during intensive training in estimating the size of steel balls all lying within a given range transferred to the estimation of size of a similar series of balls lying well outside this range?

Description of apparatus. The series of steel balls employed ranged in diameter from $\frac{16}{64}''$ to $\frac{7}{64}''$ by differences of $\frac{1}{64}''$. Ten balls of each size were used with a ball-sorting apparatus devised by Mr S. Wyatt. This apparatus consists of a box with a sloping lid.

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[1] Coefficients significant to two places of decimals only.

The box is divided into a number of compartments, the number being the square of the number of different sizes of balls employed (see Fig. 3).

In the lid are cut ten slots with sides converging in such a manner that ball 1 drops into the first row, ball 2 into the second row, and so on to No. 10 which drops into the tenth row. If the subject is then instructed to place each ball, according to size, in its appropriate groove, the balls correctly placed fall into the squares marked *X* in the figure. The number and deviation from the true position of balls wrongly placed are thus obtained directly. For example, if the subject is presented with ball 4 and places it wrongly in slot 7, it will fall in the square marked *E* in the figure.

Slots

Rows	1	2	3	4	5	6	7	8	9	10
1	X									
2		X								
3			X							
4				X			E			
5					X					
6						X				
7							X			
8								X		
9									X	
10										X

Fig. 3. Plan of ball-sorting apparatus.

Procedure and scoring. The experimental procedure was so arranged that the estimation of the size of ball should be limited to vision only. Thus the following precautions were adopted:

(*a*) The subject received the ball in a cup-shaped spoon and placed it by means of this in the desired slot—thus eliminating discrimination by touch.

(*b*) The spoon was made of such a weight as to render infra-liminal the difference between any two adjacent balls—thus eliminating discrimination by weight.

(*c*) The spoon, which was made of copper, was lined with felt in order to exclude any inference of size on the basis of the sound made in dropping.

The different steps in the performance were carried out to the beat of a metronome, the rate of which was decided by preliminary experiment.

The balls were presented to the subject by means of a feed pipe. They were in irregular order, but the same order was preserved in each test period. As the balls have a highly polished surface, care was taken to ensure constant conditions of illumination. After each judgment the subject returned the spoon to the same position, so that the ball was always viewed under the same conditions.

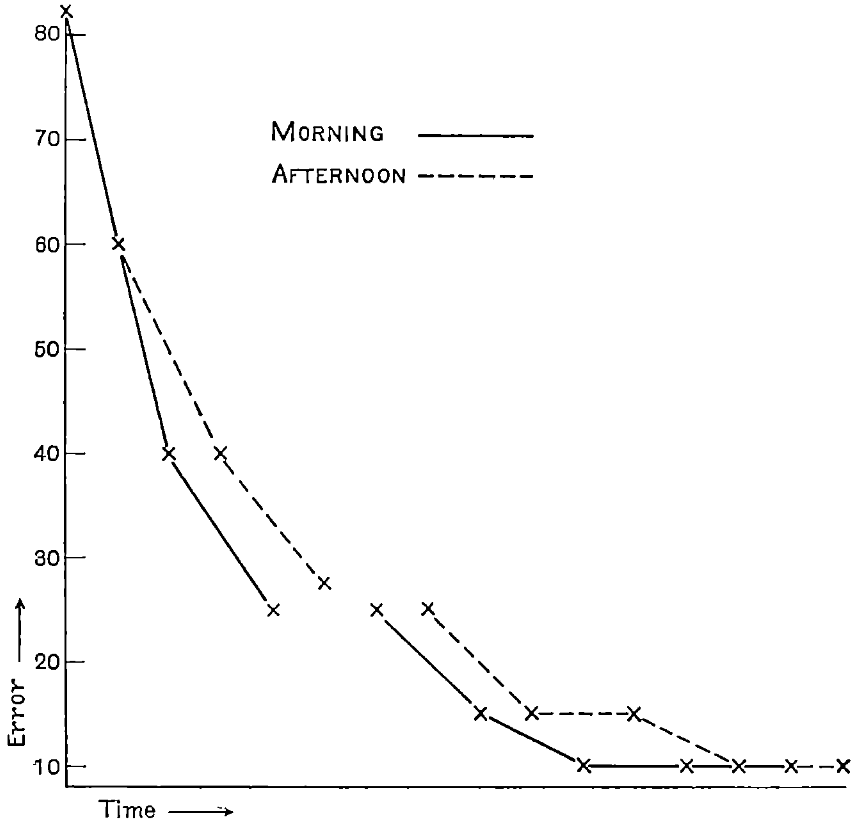

Fig. 4. Typical ball-sorting curve.

The score was taken in terms of error, measured by the total squared deviation from the true position.

The presentation of the 100 balls occupied 10 minutes; each period of 10 minutes' work was followed by 10 minutes' rest. It will be seen that the procedure minimized any personal relationship between the subject and the experimenter, thus ensuring adequate constancy in this respect.

The conditions of work and of payment were the same for these subjects as in the previously described work on Manual Dexterity. The material for the transfer tests, given on three occasions as in the experiment previously described, consisted of 100 very much larger balls[1], 10 of each size, ranging in diameter from 1″ to $\frac{33}{32}$″ by differences of $\frac{1}{32}$″. The procedure and scoring was the same as during the training. Each transfer test was of 10 minutes' duration.

The transfer test was also given under identical conditions to 18 subjects employed as a control group. Six subjects were trained in the operation for a period of 2 weeks, a satisfactory degree of practice being obtained in each case. A typical curve showing rate of improvement is shown in Fig. 4.

Table III. *Transfer. Ball sorting.*

All figures in terms of % improvement.

Subject	Initial score	1–2	2–3	1–3
Trained group				
A	265	104·6	7·4	119·1
B	343	214·1	-68·7	86·1
C	113	97·1	13·3	123·3
D	122	29·2	- 3·9	33·8
E	74	- 30·8	7·1	- 25·9
F	156	5·3	23·1	29·6
Control group				
a	165	5·1	-28·6	- 25·3
b	·76	13·4	21·8	38·2
c	252	265·2	-16·9	203·6
d	130	88·4	18·9	124·1
e	144	23·1	14·7	41·1
f	147	107·0	-22·8	59·8
g	242	163·0	2·2	168·8
h	232	81·2	-11·5	48·7
i	473	63·1	66·2	171·8
j	241	171·9	-37·3	69·7
k	278	86·6	34·2	150·5
l	237	3·1	78·7	84·2
m	204	104·0	4·2	112·5
n	137	95·7	84·2	260·5
o	194	77·9	-29·7	25·2
p	193	93·0	12·4	116·9
q	174	39·2	- 2·4	35·9
r	164	60·8	17·2	88·5

(a) *Treatment of results.*

As the number of subjects, six only, is so small, any statistical treatment is impossible. However a comparison of the results obtained from the six trained subjects with those obtained from the control group is instructive.

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[1] We wish to thank the Hoffmann Manufacturing Company for kindly lending this series of balls.

Inspection of the tables above reveals no apparent superiority of the trained over the control group. However, owing to the fact that learning is much more rapid in the earlier than in any other stage of practice, comparison of measures based on percentage improvement is legitimate only in cases where the initial score is approximately of the same magnitude.

A comparison between trained and untrained subjects, in which as far as possible this condition is observed, is shown below:

Subjects	Initial score	In % improvement		
		1–2	2–3	1–3
Trained, *A*	265	104·6	7·4	119·1
Control, *c*	252	265·2	–16·9	203·6
Control, *k*	278	86·6	34·2	150·5
Trained, *C*	113	97·1	13·3	123·3
Control, *d*	130	88·4	18·9	124·1
Trained, *D*	122	39·2	– 3·9	33·8
Control, *n*	137	95·7	84·2	260·5
Trained, *E*	74	– 30·8	7·1	– 25·9
Control, *b*	76	13·4	21·8	38·2
Trained, *F*	156	5·3	23·1	29·6
Control, *r*	164	60·8	17·2	88·5

In interpreting the above table it should be remembered that the score in the initial test is the total of the squares of each deviation from the true position. Thus, if the subject mistook ball 5 for ball 9 this added a score of 16 to his total. In consequence it seems that the cases singled out for comparison above are truly comparable. It will be seen that in each of these cases it has been possible to find a member of the control group, who effected a greater improvement in test 3 over test 1, than did a comparable member of the trained group.

Subject *B* in the trained group is omitted from the table as it is impossible to find a subject in the control group with a comparable initial score.

(*b*) *Conclusion.*

Inspection of this table leads to the conclusion that in this operation also training is specific and not general.

(4) SUMMARY OF CONCLUSIONS FROM BOTH EXPERIMENTS.

In the experiment on Transfer of Training in Manual Dexterity, no case was found in which the difference between trained and control groups, with regard to the improvement in the third over the first application of a test, was of such a size as to exclude the possibility of its being due to chance factors.

In some cases, the brief practice afforded by the test itself is shown to be more effective than the intensive training in an analogous performance.

The results of both experiments lead to the conclusion that in manual dexterity and in visual discrimination, the effect of training is specific rather than general.

(*Manuscript received* 22 *December* 1927.)

CONCERNING SUBJECTIVE JUDGMENT OF DIFFICULTY

BY ERIC FARMER[1].

DURING some recent experiments an opportunity was offered of testing the relationship between thinking a task difficult and actually doing it badly.

The apparatus used was a Pursuit Meter designed by Dr Edgar Schuster. It had a mechanically-driven pointer which when set in motion moved in an irregular manner for one minute, during which the subject had to keep another pointer, controlled by his master hand, as near to the mechanically-driven one as possible. Any divergence in the position of the two pointers in excess of two millimetres was automatically recorded. The apparatus permitted of four different patterns being described by the mechanically-driven pointer, these forming four separate tests known as tests *A*, *B*, *C* and *D*.

Before giving the tests to a large number of people a laboratory experiment was carried out to determine the relative order of difficulty of the four tests. The results of this experiment placed them in the order *C*, *B*, *D*, *A*—*C* being the easiest and *A* the hardest. In the field experiment on three groups of boy apprentices, comprising in all 428 individuals, the tests were given in this order so that any improvement due to practice would show itself by enabling better scores to be made in the harder tests. The relative order of difficulty of the four tests, as measured by the performance of the three groups, turned out to be the same as that arrived at by the laboratory experiment (see Table I, columns 1 and 2), and since the factor of improvement was working against this result we can feel some confidence in accepting it as a real measure of the relative difficulty of the four tests.

Each subject, after he had done the tests, was asked to say which one he found the most difficult[2]. In order to measure the value of their

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[1] I am greatly indebted to Miss E. M. Newbold for the statistical part of this paper.

[2] The actual question put to each subject was, "Which of the four tests did you find the hardest?" It was impossible for the subject to know what his actual score in any test was, and there can be little doubt that in so far as his answer was not determined by chance it expressed a judgment of subjective difficulty and not an attempted estimate of his objective performance.

Table I.

(1) Tests in order of actual difficulty (*i.e.* by observed performance)	(2) Actual average performance score. (High score indicates poor performance)	(3) % of people who *estimated* this test to be the most difficult	(4) Difference from 25 % (*i.e.* % expected from random estimate)
	All groups (428 subjects).		
A	74	36·4	11·4 ± 1·4
D	64	35·5	10·5 ± 1·4
B	55	18·2	− 6·8 ± 1·4
C	51	9·8	− 15·2 ± 1·4
	Group *X* (82 subjects).		
A	74	36·6	11·6 ± 3·2
D	64	37·8	12·8 ± 3·2
B	54	22·0	− 3·0 ± 3·2
C	50	3·7	− 21·3 ± 3·2
	Group *Y* (88 subjects).		
A	68	28·4	3·4 ± 3·1
D	61	53·4	28·4 ± 3·1
B	52	14·8	− 10·2 ± 3·1
C	50	3·4	− 21·6 ± 3·1
	Group *Z* (258 subjects).		
A	76	39·1	14·1 ± 1·8
D	65	28·7	3·7 ± 1·8
B	57	18·2	− 6·8 ± 1·8
C	52	14·0	− 11·0 ± 1·8

answers the first thing to do is to see if they were given at random. This was tested in the following way. In any group whose members answered at random we should expect to find one-quarter of the group who would say any particular test was the hardest. The difference between the expected number and the observed number is a measure of how far the subjects' answers were determined by chance. Table I, column 3, gives the results for each test.

In the majority of cases differences between the expected and observed number who say any one test is the hardest (Table I, column 4) are in every group significant. In tests *A* and *D* (the harder tests) the observed number is significantly greater than the expected, and in *B* and *C* (the easier tests) the observed number is significantly less. This means that a significantly larger number of people thought the harder tests harder, and that a significantly smaller number of people thought the easier tests harder than can be expected by chance. From this it is certain that the answers were not determined merely by chance, and we can go on to consider two other points: (1) how far the group estimate of difficulty corresponded with difficulty as measured by the group performance, and (2) how far individual estimates of difficulty corresponded with individual performance.

(1) In all groups (Table I) the percentage of those who chose *C* as being the most difficult is smaller than the percentage of those who chose *B*, and the percentage who chose *B* is in all groups smaller than those who chose *A* or *D*. Since *C* and *B* were the easier tests there is so far agreement between the group judgment and the group performance. Except in group *Z* the relative difficulty of *A* and *D* as measured by performance and judgment do not agree, so that there appears to be greater difficulty in distinguishing between the relative difficulty of the two harder tests than between that of the two easier tests.

Table II. *Separate tests. The actual performance of subjects (classified according to their estimation of the difficulty of the tests).*

		Percentage of the given class whose performance in the given test was in the following order relative to the other tests			
Class	Test	Worst	2nd worst	3rd worst	4th worst
P (156)	*A*	79·2 ±2·6	16·4 ±2 4	3·8 ±1·4	0·6 ±0·6
Q (272)	*A*	57·4 ±2·0	31·7 ±1·8	9·4 ±1·1	1·5 ±0·4
All (428)	*A*	65·3	26·1	7·4	1·2
P (152)	*D*	39·8 ±2·5	43·4 ±2·7	13·5 ±2·1	3 3 ±1·2
Q (276)	*D*	23·7 ±1·9	47·9 ±2·0	21·7 ±1·6	6·6 ±0·9
All (428)	*D*	29·4	46·3	18·8	5·4
P (78)	*B*	0·0 ±1·4	25·2 ±3·1	42·5 ±3·7	32·3 ±3·7
Q (350)	*B*	4·3 ±0·7	19·4 ±1·5	36·1 ±1·7	40·2 ±1·8
All (428)	*B*	3·5	20·4	37·3	38·8
P (42)	*C*	7·1 ±1·5	6·7 ±2·7	22·2 ±5·0	63·8 ±5·2
Q (386)	*C*	1·6 ±0·5	7·2 ±0·9	36·6 ±1·6	54·7 ±1·7
All (428)	*C*	2·1	7·1	35·2	55·6

Class *P* is composed of those who *estimated* the given test to be the hardest. Class *Q* is composed of those who *estimated* some other test to be the hardest.

(2) In Table II the subjects are divided into two classes, *P* and *Q*, separately for each test. Class *P* is composed of those who ***think*** the particular test is the hardest, and class *Q* of those who ***think*** some other test the hardest. The percentage in both these classes who *do* the particular test worst, second worst, third worst and fourth worst is also given. The last line in each section shows the percentage of the whole group whose performance of the particular test falls into these grades. These percentages may be taken as those expected in the classes *P* and *Q* if the estimation were purely random, and the probable errors given have been based on them.

In the case of tests *A*, *D* and *C* the percentage of those who estimate them to be the hardest and actually perform them worst is larger than would be expected if the estimate were purely random. In tests *A*, *D* and *B* (though in the latter two the difference is not significant) the percentage of the same people perform the test *best* (*i.e.* fourth worst) is smaller than would be expected on a random estimate. On the other hand, not one of those who thought *B* the hardest actually did it worst, and the percentage of those who estimated *C* to be the hardest and actually did it *best*, is larger than would be expected from a random sample of the whole population. The people who estimate *B* or *C* (the easier tests as measured by performance) to be the hardest are worse judges of their performance than those who estimate *A* or *D* (the harder tests as measured by performance) to be the most difficult.

Table III. *Separate tests. Judgment of subjects (classified according to their actual performance in the given test).*

Class	Test	Percentage of the given classes who *estimate* that the given test is the most difficult
(280). Those who *do A* worst	*A*	44·2 ± 1·0
All (428)	*A*	36·4
(126). Those who *do D* worst	*D*	48·0 ± 2·9
All (428)	*D*	35·5
(15). Those who *do B* worst	*B*	0·0 ± 6·7
All (428)	*B*	18·2
(9). Those who *do C* worst	*C*	33·3 ± 6·7
All (428)	*C*	9·8

In Table III the results are shown the other way round, the subjects being classified according to their actual *performance* in each test, and the *estimates* of those who do the given test worst are compared with the estimates of the whole group. (The probable errors are based on the latter figures.)

In every test except *B* those who *do* any test worst have a larger percentage among them who *estimate* that test to be the hardest than is found in the whole group. In *B* not one of those who *did* it worst *thought* it the most difficult, but the probable error here is very high.

CONCLUSION.

Taking the results as a whole there is a certain amount of agreement between group performance and group estimate, but it is by no means a striking one. Difficulty was found in distinguishing the relative hardness of *A* and *D*, though they were always correctly rated as harder than *B* and *C*. This may be due to the smallness of the difference in difficulty between the two tests.

None of the subjects who *do B* worst think it the most difficult, and an unexpectedly large proportion of those who *think C* the hardest *do* it better than any other of the tests. These two tests are the easiest to do and are estimated by the three groups as being so. Those therefore who think either of these tests the hardest or do either of them the worst differ markedly from the whole group and may be regarded as 'peculiar' in some way. It is not surprising, therefore, to find that there is a marked divergence between their own individual performances and judgment of difficulty. Those who differ most from the normal are the worst judges of their own performance.

(*Manuscript received* 13 *September* 1926.)

SCIENCE AND ETHICS

A PSYCHOLOGICAL STUDY

By EUGENIO RIGNANO.

The distinction between the function of science and that of ethics should nowadays be perfectly clear. Distinct as they are one from the other, there ought to be no doubt as to the mutual relations which yet unite these two activities of the spirit.

This, however, is not the case, and one is apt to fall into one or the other of two opposite extremes: in the first, continuing to believe that science can dictate by itself the norms of rational morals; in the second, maintaining, instead, that no relation whatsoever can exist between science and ethics, precisely because of their fundamental difference.

The aspiration to found a rational moral system is as old as Philosophy itself. From Socrates and the ancient Stoics to Kant, this great hope has never been abandoned. It is easy to comprehend why this should be the case. Considering, in fact, the unanimous agreement which the conclusions of logical reasoning obtain, can we doubt that, were one able to demonstrate by means of reasoning, the 'truth' of certain fundamental ethical principles, the unanimous agreement regarding such principles, evoked by this demonstration from all beings endowed with reason, would *ipso facto* be reflected in their conduct? Humanity, in a bound, would then rise to such a high level as not even the most advanced Utopians dare to-day hope for, even in a remote future.

The longing for 'rational' ethics and the tenacious belief in the possibility of realizing such a dream are the results of the great gap which, till now, Psychology has allowed to remain in the analysis of the phenomena of the spirit—a gap, which, owing especially to the English Associationistic School, many psychologists still fail to fill up. In fact, the insistence upon explaining all the phenomena of the human spirit through sensation alone, and through the relations between sensations and sensations, has, till now, hindered us from having a clear vision of the other fundamental activity of our mind, which exists along with the purely sensory one, and is, indeed, the real and only driving force of our psychical being. And this has prevented our perceiving in what reasoning really consists, from what its objectivity is derived, why every rational being must necessarily adhere to its conclusions, and why, from its very nature, it could never be extended so as to 'demonstrate the truth' of any fundamental ethical principle.

My investigative method of thorough analysis, preceded, guided and carried on by synthetic intuition, applied also to the psychical field, has proved to me, as is known to all who have followed me in my studies, that all the phenomena of our mind—from the simplest to the most complex—are always the result of the interplay of two distinct kinds of elementary psychical phenomena, beyond which psychological analysis cannot advance. These represent the two fundamental primitive materials of which any psychical edifice is found to be composed. These are the affective tendencies on one side, and the sensations and sensory evocations on the other. The former constitute the impelling and determining agent of any manifestation whatsoever of thought, of any activity of the spirit, of any manifestation of the will, of our every action, and of all our behaviour in general. The latter constitute the simple material of a representative order, stored in our mnemonic accumulations, and acting, in respect to the affective tendencies, as a varied complex of symbols, direct or indirect, referring to situations of environment, immediate or future, longed-for or feared, and able to illuminate such affective tendencies as to the path to be taken in order that we may reach the goal or goals towards which they tend by an intimate urge of their own.

By this analysis I found that reasoning is, in its origin and development, only a series of experiments, or actions, merely thought rather than carried out in effect, which this or that affective tendency anticipates in thought, precisely in order to prove, before effectuating them, whether such acts would lead to the desired goal or succeed in avoiding the situation feared in a special environment.

From this nature and function of reasoning is derived its 'objectivity,' *i.e.* the unanimous agreement with which all its conclusions are necessarily received. In fact, it being only a series of experiments, merely thought, to each of which is attributed the result which an identical experiment, effectively carried out, has produced in the past, it is consequently definitively based on empirical proofs of a perceptive order, the unanimous assent to which is therefore reflected upon each of its conclusions. Hence, I repeat, the unanimous assent to all logical demonstrations in general; hence, the unanimous acknowledgement of scientific laws or 'truth,' whether directly discovered and proved empirically, or indirectly derived by logical deduction from a combination of other empirical discoveries; hence, briefly, the 'objectivity' of reason and science[1].

The task of science, therefore, is only that of explaining phenomena; that is, of deriving the greatest number possible of these, by logical deduction, from the smallest number possible of other phenomena, ascertained by direct observation. That is to say—especially when we pass from concrete to abstract reasoning, which covers the same ground as thousands and thousands of particular concrete reasonings taken together—that the task of science is limited entirely to describing the greatest number possible of natural phenomena in the most concise and exact way possible. (Even the Newtonian formula of Universal Gravitation, with all the deductions drawn from it by pure reasoning, is but a concise mode of describing all possible celestial movements, all the forms of the stars and planets, all the tidal phenomena, and so forth[2].)

When, on the other hand, from empirical proofs of a perceptive order identical for all individuals, and from the reasoning founded on them, which reaches conclusions accepted equally by all endowed with sound reason, we pass to the affective valuation of this or that fact, to the affective motives which prompt action, in rapport with a given environmental situation, in such and such a way, the unanimity ceases *ipso facto* and the 'objectivity' of the empirical proof and of reasoning is followed by the 'subjectivity' of this affective valuation and of this or that motive impelling to action.

In my study of the origin and mnemonic nature of the affective tendencies, I found that their 'subjectivity' depends on the fact that the individual possesses some or other idiosyncratic affective tendencies, some or other nostalgic feelings, according to difference of surroundings, to various particular relations in the special environments, to which the individual and his forefathers have been subjected for a longer or shorter time in the past, and to which they have grown more or less accustomed; that is, according to the particular history lived by each individual and his ancestors.

This mnemonic and historical origin of all the affective tendencies explains thus the 'subjectivity' and infinite diversity manifested by individuals in their needs, appetites, desires and aspirations, and, by reflection, in all 'affective valuations.'[3]

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[1] See Eugenio Rignano, *Psychology of Reasoning*, chap. IV: "What is Reasoning?" London: Kegan Paul, Trench, Trubner and Co., 1923.

[2] See Eugenio Rignano, *ibid.* chap. V: "The evolution of reasoning. From concrete to abstract reasoning."

[3] See Eugenio Rignano, *ibid.* chap. I: "On the mnemonic origin and nature of the affective tendencies."

This affective subjectivity being admitted, it can be seen that reason and science have, at first, but one plain and clearly limited task, recognized and acknowledged by all: namely, that of indicating the means through which this or that wished-for end can be reached. By thus indicating what path should be followed, reason and science may certainly influence human conduct, so much so as sometimes to transform it completely, while the affective tendencies impelling to their respective end remain unchanged. Thus, for instance, science, dissipating certain superstitions, causes magic practices to cease which had as their end to preserve from certain diseases. One aims at this no less than before, but the methods used are now more rational.

But the affective tendencies which are stored up in a potential state in one and the same person are, owing to their origin and their nature itself, almost innumerable, we might say infinite, if we take into account all—even the slightest—of their nuances. Now some of these awake, now others; some become active contemporaneously now with certain others, now with others still, differing from the former. And in this contemporaneous activity of theirs they flow together, blend, strengthen one another, or, on the contrary, interfere with, modify, hinder one another reciprocally, and thus continually producing resultant affectivities, extremely complex, capable in the several cases of differing in small gradations, according to the number, quality and intensity of their respective components[1].

Hence *that inconsistency in behaviour* made possible by the fact that now one affective tendency impelling to given actions becomes active, now another, and that the second may urge an individual to try to avoid the consequences of the actions impelled by the first. From this arises remorse, repentance, discontent of the soul, whenever the results arising through the activity of one given passion awake an opposite feeling no less strong, and consequent acute pain in ascertaining such results. Thus, the passion for gambling throws many individuals into despair, when they become aware of the misery into which they have brought their families whom yet they tenderly love. The passion for a mistress instigates to robbery even individuals, who, later, feel horror at the idea of having committed such an act. The unashamed passion for some vice or other causes many to ruin their health, which they would yet fain preserve.

Now, it is precisely here that the second of the three spheres of action in which reason may influence human conduct, opens out, and widens more and more. In logically inferring the consequences of a given act—and the stronger the ratiocination, the more evident will they be—it happens that *at the very moment in which one or other of the affective tendencies impelling to such an action comes into play, there awakes also the opposite affectivity which tends to avoid the consequences of this action.* Instead of leaving it undisturbed and free to carry out its work—as happens when the other tendency anxious to obviate the results of such an action only awakes when such results under the urge of the first tendency are irremediably obtained—reasoning provokes the immediate and contemporaneous awakening of the antagonistic affective tendency, tending to inhibit the former. And the more evident—owing to the force of the ratiocination itself—does the future result of the action, to which the former urged, appear, so much greater intensity does the latter opposing it acquire, and so much more probable does its victory over the former become: sterile, posthumous remorse is thus substituted by a fecund, timely inhibition, which obviates all tardy and futile regret.

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[1] See Eugenio Rignano, *ibid.*

Through such continual exercise of reason, man tends to render his behaviour more and more consistent in respect to the profoundest and most tenacious of his affections, which best represent and constitute his particular personality. That is, he succeeds in the timely inhibition of those secondary, more transitory, affective tendencies which would lead him to act in opposition to the fundamental ones. Thus, at last, he deprives these secondary affections even of the possibility of re-awakening, and manages to uproot them altogether. The fundamental affections finding no longer any opposition, he is no longer subject to remorse or repentance, and thus attains the calm serenity to which the Stoic aspired.

Besides, with the intensification in him of the rational need—which becomes predominating through the continual, daily practice of reason, as is verified especially in thinkers and scientists—there arises in him and becomes ever livelier the desire of unifying his most fundamental affections themselves, that is, of co-ordinating them according to one supreme end, of which they should be but so many aspects, and obtaining this by modifying and toning them down till they are in harmony with it.

Thus arises, in a man who is morally and rationally superior, the ardent longing to carry on the research of a *supreme ethical postulate,* from which to derive all the motives of his conduct.

This supreme ethical postulate, however, can itself be only the resultant of the most fundamental affective tendencies of the individual, who longs for it.

Reason can afford valid assistance only in clearly pointing out even the remotest consequences to which each of such tendencies would lead, if left free to explicate itself completely, and in causing them, therefore, when called contemporaneously into activity, to modify one another reciprocally and harmonize with one another precisely where, otherwise, they would lead to future mutually contrasting consequences. But this being accomplished, the task of reason is over. Having awakened all the fundamental affective tendencies at once and pointed out to them all—even the remotest—consequences of each of them, reason has now but to retire and allow to be formed from this simultaneous presence of differing affective tendencies, by means of composition, modification, reciprocal partial inhibition, etc., that resultant affective tendency, whose end will be expressed precisely by the supreme ethical postulate, dictated therefore, in each individual, not indeed by his reason, but by all his affective substratum.

The subjectivity, therefore, characteristic of the affective tendencies, persists also in the supreme ethical postulate, thus assumed. We have but to recall the variety of the supreme ethical postulates of the several philosophical schools of ancient Greece; the differences between the Stoic doctrine, aristocratically somewhat cold and stiff even in its great moral sublimity, and the spirit of such warm love and deeply-felt compassion for the misery and suffering of others, which emanates from the Gospel; or the great dissimilarity between the Buddhistic morality and the very practical ethics of Confucius; and how, almost in the same epoch and in the same social environment, a Nietsche appeared along with a Tolstoi.

With the advance of civilization, however, society tends to stereotype more and more the behaviour of a larger number of its members, in order to render it more and more adapted to the best social life possible.

If however this increase of exterior moral uniformity is due exclusively to the social sanction of reward and punishment, which becomes even more perfect and efficacious, it will be based only on individual interest; and even if this represents great social progress and is of immense social advantage, yet it does not deeply modify the intimate moral nature of the individuals, which will be always inspired by egoistic sentiments alone.

The modification, on the contrary, may be profound and essential, if caused by really new moral affective tendencies, or even by new blendings or sublimations of those already existing, which education may, in always increasing measure, inculcate in the heart of the young, and which may be caused to blossom even in the adult by the apostle, if the general dominating conceptions on the whole of nature and the world happen to be favourable to him.

Here it is that science may step in and claim her task. It is a task, not only for psychological science, in illuminating and guiding with ever greater wisdom the art of education, but for the whole of science in general, whose duty it is to widen more and more the horizons opening before the affective psyche of the individual, to present to him this or that *Weltanschauung*, or general conception of the Universe, and thus to offer him an increasingly large field for new affective 'transferences' (Ribot), for new blendings and sublimations of feelings, formerly concentrated on narrower horizons. This, indeed, is the third and most important form of influence which science can justly claim to exercise on morals.

From this point of view the *Weltanschauung* which science can provide on the nature of life is of fundamental importance. Similarly important is the definite reply to the question regarding the existence or non-existence of the finality of life, of which man feels himself, at the same time, a small and yet a great part.

Very different, for instance, is the action exercised by science on the elevation of human morals according to its acceptance or refusal of the mechanical conception of life, which admits even in life itself only phenomena of a purely physico-chemical nature. In fact, the belief that man is nothing but one of the many fortuitous, insignificant links in a perpetual physico-chemical transformation with no goal, as brute matter, must needs constitute a depressing factor, weakening every aspiration and effort towards self-elevation. Man has in himself the irresistible intuition that the life throbbing in him is something essentially different from what takes place in a laboratory retort; he profoundly feels that all in his being is finalistic. And if science, on the basis of irrefutable facts (to which the followers of the mechanical conception of life wilfully close their eyes), succeeds in proving that life is a peculiar cosmic activity, with a finality of its own, which, precisely because it is finalistic, is profoundly detached from and opposed to the essentially non-teleological physico-chemical world, a wide field will be offered to ethics to restore and strengthen the feeling that life has been bestowed upon us to the end that some aim should be accomplished.

Different, also, will be the influence exercised by science on the moral elevation of man according to whether or not it proves that, with the material death of the individual, all that was a fundamental part of his spirit, all this ego, will cease or whether it can give him the more consoling surety that the best and most representative part of his ego may aspire to a sort of real immortality. By this I mean the assurance that, even after material death, for what concerns those activities in which his noblest aspirations were expressed, the individual will endure as a factor of good in the most distant future, for all humanity that comes after him. This immortality, for spirits already morally superior, is all that is to be desired.

Finally, the influence exercised by science on the moral elevation of man, will be different in so far as it proves or not that pretended human progress is nothing but a great, ingenuous illusion, proceeding by chance now in one direction, now in another, often quite opposite, so that it is a vain toil of Sisyphus to spend one's life in urging progress forward in the direction desired—or whether it demonstrates that life, in its countless ages of evolution, really tends, although certainly with ebbs and flows, towards one clearly determined goal, that is to pass from the furious primitive clash of single finalisms to an ever less imperfect adjustment of these with one another, thus substituting for the painful struggle of life life's joyous harmony[1].

If harmony is shown to be the ultimate and definite goal which each and every group of living beings has always striven to reach; if this is shown to be the ultimate profound finality which, even when it is unconscious, binds together the whole of life in fraternal solidarity, and which at last becomes reflected in human society and finds its adequate and conscious expression in the two great social aspirations—justice and morality—then a wide field will be afforded to every apostle of the good, that he may, by means of new affective 'transferences' and 'sublimations,' direct the ordinary affective tendencies of the mind towards this finality of life. Thus, he will bestow new hopes and a fresh and persevering tenacity upon mankind in its steep and painful ascent towards the shining goal.

At last (by the unanimous, universal agreement, which it cannot fail to obtain, as the finality of all life, which in man has risen to consciousness, urges towards it more and more pressingly and imperiously), this supreme ethical postulate of harmony among all living and breathing creatures—this postulate in which are summed up and blended all the noblest principles which honour humanity and have been proclaimed by the most fundamental moral doctrines—will, for the first time, acquire that very character of absolute eternal truth which to-day appertains only to the dicta and laws of reason and science.

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[1] See Eugenio Rignano, *Qu'est-ce que la vie?* chap. XII: "Conclusion: The place of man in the universe and the new morals of the harmony of life." Paris, Alcan, 1926.

(*Manuscript received* 20 *September* 1927.)

NOTE ON THE NATURE OF INTELLIGENCE

By C. BLACK.

1. Intelligence is essentially dependent on the innate dispositions, *i.e.* the instincts, the 'engram-complexes' of ordinary perception of the external world[1], and the 'engram-complexes' of Reason, Ethics and Aesthetics[1]. Probably we ought also to include the 'general tendencies' (*i.e.* sympathy, suggestion, imitation, temperament)[2], and Memory as a separate characteristic of all dispositions.

2. An Instinct involves an untaught ability to appreciate the meaning of some external thing or event[3]. If a particular instinct works well, *on the side of appreciation of meaning*, its possessor is, in so far, intelligent. Even if he is unsuccessful on the action side of the instinct, he is still intelligent in respect of that *meaning*; though he may be said to be weak or impotent—or self-controlled—on the action side. Thus an animal who through disablement was unable to run away from danger would show by other symptoms that it appreciated the danger and wanted to run away; and no one would call it unintelligent because it did not succeed in doing so.

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[1] See Nunn's *Data and Principles of Education*, chapters IV and XIV.

[2] See McDougall's *Social Psychology*, chapter IV.

[3] This assumes McDougall's Theory of Instinct as against the Behaviourist one. That Instinct involves 'appreciation of meaning' is not explicitly stated in McDougall's definition, but it is implicit in it, and is explicitly stated by him in the chapter (II) of his *Social Psychology* which contains the definition. Thus (p. 26): "There is every reason to believe that even the most purely instinctive action is the outcome of a distinctly mental process... that is to say, every instance of instinctive behaviour involves a knowing of some thing or object, a feeling in regard to it, and a striving towards or away from that object." And again (p. 28): "The impression must be supposed to excite not merely detailed changes in the animal's field of sensation, but a sensation or complex of sensations that has significance or meaning for the animal."

The phrase 'appreciation of meaning' is purposely vague—inevitably so in the present state of psychological knowledge—and must not be taken to imply that instinct necessarily involves ideation; the inference is rather that appreciation of meaning can take place on the lowest animal level without the aid of ideas or mental imagery. Probably, even in human beings, much—and perhaps the most important instances of—appreciation of meaning takes place by means of mental processes much more primitive than imagery. The conception of the engram-complex—used in the later paragraphs of this note—is an attempt to describe such primitive and fundamental processes.

Since the appreciation of meaning is almost identical with experiencing the emotion of the instinct, it is essentially untrue to say that Intelligence is unconnected with Emotion. (See the B. of E. Report, "Psychological Tests of Educable Capacity," p. 72.)

A priori, a person or animal might be intelligent in respect of one instinct and unintelligent in respect of another. If, taking account of all the instincts, the majority worked well, the person or animal would, *on the whole*, be intelligent. Actually it may be that all the instincts must necessarily work well or all ill. (This corresponds on the level of instinct to the point in dispute between the schools of Spearman and of Godfrey Thomson.)

This view of Intelligence—so far as it concerns the instinctive level—corresponds, I think, with the popular use of the word, which would distinguish between 'intelligent'—a term applicable to animals—and 'intellectual.'

3. An Engram-Complex relating to perception of the external world again involves appreciation of meaning. (See Nunn's *Data*, *loc. cit.*) If these engram-complexes work well, their owner is again, *on the whole*, intelligent.

It is a question whether these might work well and the Instincts badly, and *vice versa*; and whether their working is independent *inter se*.

It is doubtful whether this element in intelligence enters into the popular use of the word to any extent. But, accepting for the time being the concept of the engram-complex, the psychologist can see that any person or animal which had weak engram-complexes of this class would inevitably act in a way which would be dubbed unintelligent by the common man.

4. The engram-complexes of Judgment—of the three classes, Reason, Ethics and Aesthetics—again involve meaning; and if they work well will add to the general intelligence of their possessor—*i.e.* to his intelligence *on the whole*.

Whether the engram-complexes of one class necessarily all work well or ill is again a moot point—and specifically the point at issue between the Spearman and Thomson schools. And the same holds good as regards the relation of one class to another; and as regards the relations between these three classes and the elements dealt with in sections 2 and 3 above.

The popular usage of 'intelligent' and 'intellectual' merge in one another here.

5. The position of the 'general tendencies' is doubtful as regards meaning.

Sympathy might be said essentially to involve appreciation of meaning—perhaps of a fundamental kind (*i.e.* of ontological unity). But Suggestibility might be said essentially to involve absence of appreciation of meaning. And Temperament is probably independent of appreciation of meaning.

Nevertheless, one whose disposition of 'Sympathy' was weak or worked ill, might properly be called unintelligent in that respect, though popular usage would employ the term 'selfish.'

Temperament is more difficult to deal with. An irascible temperament might disturb the proper working of the other 'Dispositions' and so produce unintelligence. It is difficult to say whether popular usage would call such a one unintelligent or merely irascible or 'cussed'; probably the last.

6. Memory is a characteristic of all the 'dispositions' and affects both their perceptive aspect (appreciation of meaning) and their responsive aspect (action). Its relation to the dispositions is thus in a sense similar to that of Intelligence itself; but since it affects efficiency of appreciation of meaning, it ought to be regarded as standing in a casual relation to Intelligence.

7. On the whole, the best definition of Intelligence as popularly used would seem therefore to be that it is the average efficiency of the various innate dispositions, on the side of meaning or perception; *i.e.* excluding efficiency of reaction.

More scientifically—*i.e.* in order not to prejudge the Spearman *v.* Thomson issue—every innate disposition has its own coefficient of efficiency (on the side of meaning or perception); that is to say, there are as many intelligences as there are innate dispositions, and these may or may not be innately connected.

8. The restriction of intelligence to the side of appreciation of meaning and explicitly excluding efficiency of reaction, is analytically essential; as is, I think, made sufficiently clear by the example of the disabled animal unable to run from a danger it clearly apprehends. If in practice a test of intelligence—whether a popular (observational) one or a scientific (experimental) one—is generally one which tests the reaction, it is because it is generally assumed, as a result of general observation, that the two things go together. It is however theoretically possible that in connection with any instinct or engram-complex (or group of either) the appreciation of meaning mechanism and the reaction mechanism might have distinct efficiencies; that one might be relatively efficient and the other relatively inefficient. And this is probably to some extent the case

in all persons and animals, and with all instincts and engram-complexes. Thus one animal which was quicker or more discriminating than a second in the appreciation of danger might run away less quickly than the second, that is, react less efficiently on the whole, but only because it had a poorer muscular equipment.

A test of reaction or 'adaptability' gives therefore a coefficient of efficiency which is compounded of the two coefficients of efficiency, (1) of appreciation, (2) of reaction. This is implicitly recognized in various places in the B. of E. Report "Psychological Tests of Educable Capacity," *e.g.*

> (1) (p. 77): Secondly, the test assumed that the child had made as much use as his 'intelligence' had permitted of stimuli afforded by such an environment. He might not have done so. His low rating on the 'intelligence' scale might then be due in part to an abnormality of temperament.
>
> (2) (p. 80): In addition to the two essential items—his inherited 'intelligence' and his chronological age—his performance in the tests would be affected by numerous subsidiary conditions, such as industry, goodwill, keenness, emotional stability, information acquired at school, social environment, and sex, which would inevitably impair or improve the result.
>
> (3) (p. 91): Again, group tests implied to a considerable extent the use of pen or pencil in writing, drawing, underlining, etc. When speed of performance was used as a measure of efficiency an error in rating might creep in, for all pupils did not handle pen or pencil with the same ease and facility.
>
> (4) (p. 92): Several witnesses thought that the interview, so far as it related to children of school age, was as at present conducted, frequently rather a test of social opportunity and upbringing than of intellectual capacity.
>
> (5) (p. 94): But, as Binet pointed out in several of his articles, the tests may yield unsatisfactory results owing to peculiarities in the character and temperament of those to whom they are applied. For example, it is a difficult matter to apply them satisfactorily to highly nervous children....

In all these instances there is implied a distinction between efficiency of appreciation of meaning and efficiency of reaction, and it is recognized that the test of intelligence may fail because of the intervention of the second factor. Nevertheless, the assumption that the two efficiencies generally go together is probably justified: otherwise it would be impossible to form judgments of the 'intelligence' of other people, since we can generally only judge by their reactions. But while this consideration may justify observational estimates of 'intelligence' it should not be taken to condone the confusion of the two kinds of efficiency in scientific (experimental) tests of 'intelligence.' The experimental psychologist must try to invent tests of appreciation of meaning proper, excluding so far as possible the element of reaction, or allowing for it in some way in interpreting his results.

(*Manuscript received* 20 *August* 1927.)

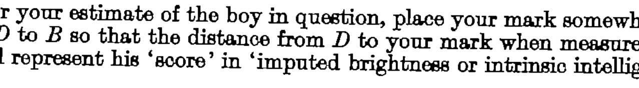
ON THE CORRELATION OF INTELLIGENCE TEST SCORES WITH IMPUTED INTELLIGENCE

By LUTON ACKERSON, *Illinois Institute for Juvenile Research*, AND GEORGE H. ESTABROOKS, *Colgate University*.

THIS is a report of the study of the correlation of the scores from a typical group test of intelligence with teachers' ratings of intelligence, together with a consideration of the general magnitude of this correlation so far as it may be inferred from the data of one investigation.

Two forms of the Otis Advanced Examination[1] (*A* and *B*) were administered in March 1923 and May 1924 to 89 boys of the Oxford (England) Central School, whose ages at the time of the second testing ranged from 11 to 15 years, with a mean age of 12·44 years and a standard deviation of 1·01 years. The mean I.B., *i.e.* the 'index of brightness,' obtained in a manner which is unique to the Otis test, was 123·26 for the first test and 125·30 for the second test, with standard deviations of 20·63 and 25·16, respectively. (Transmuted into terms of the more familiar 'I.Q.'s' by the method noted in the Otis manual of directions, the means would be not far from 109 and 110 respectively.)

Two masters in the school made two ratings each upon all the 89 boys, so that in all each boy received four single ratings. The first ratings, obtained in May 1924, were by the 'graphic' or 'cross on a line' method, which is sufficiently described in the accompanying fac-simile of a rating sheet.



INSTRUCTIONS. For the pupil whose name appears on this sheet make an estimate of how 'bright' he is for his age in all of his behaviour, regardless of his actual success in his studies. If he is the 'dullest' schoolboy for his age you have ever seen, place his mark at the very end of the line at *D* (meaning 'dullest'): if he is the 'brightest' schoolboy for his age you have ever seen, place his mark at the *B* (meaning 'brightest'). The theoretical 'average brightness' would be denoted by a mark at the exact middle point of the line.

Now for your estimate of the boy in question, place your mark somewhere on the line from *D* to *B* so that the distance from *D* to your mark when measured in millimetres will represent his 'score' in 'imputed brightness or intrinsic intelligence.'

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[1] *Otis Group Intelligence Scale, Advanced Examination*, Harrap, London.

The second ratings, obtained about six weeks later, were by the 'order of merit' method in which the rater ranked the boys, whose names were written on separate slips of paper, from 'dullest' to 'brightest.' These rankings were transmuted into 'sigma units' or 'standard measures[1]' by means of Thorndike's[2] Table 54 in order that Pearson product moment correlation coefficients could be computed on all the measures in this study. This change in rating device was made in an attempt to reduce the likelihood of the second series of ratings being merely a remembered copy of the earlier series.

Some of the correlations are presented in the accompanying table. The letters *A* and *B* (in connection with the ratings) denote the two raters, the headmaster and one of his assistants respectively. The numbers 1 and 2 denote the first and the second ratings respectively. Such measures as 'Ratings *A* 1 + *A* 2' denote the sum or average of the two ratings when these are considered as reduced to standard measures[3] in order that each series of ratings may be weighted equally regardless of the differing variabilities among different raters' ratings.

Among the correlations one notes that the mean of the four intercorrelations of one rater's ratings with the other rater's ratings was 0·665, with a range of 0·642 to 0·681, and that the mean of the eight correlations of a single rater's ratings with a single Otis test series was 0·556, with a range of 0·505 to 0·602. The correlation of the mean of the two ratings by one rater with the mean of the two ratings by the other rater was 0·737 ± 0·033, and the correlation of the mean of all four one-rater ratings with the mean of both forms of the Otis test was 0·657 ± 0·041.

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[1] Kelley, T. L., *Statistical Method*, Macmillan, 1923, pp. 114–117.

[2] Thorndike, E. L., *Mental and Social Measurements*, Teachers' College, Columbia University, New York, 1913.

[3] In a study of this sort in which one is interested only in the *correlations* of averages of measures and not in the average values themselves it is not necessary to go through the labour of transmuting the original measures. Greater accuracy as well as greater economy may be attained by computing the unitary correlations upon the original measures and then calculating the correlations of the averages by means of the useful Spearman sums-and-differences formula (see this *Journal*, v, 419, or Kelley, *ibid.* No. 147), which in this case becomes delightfully simplified since all standard deviations may be put equal to unity. For example, the briefest form, that for the correlations of one variable with the sum or average of two others, *i.e.*

$$r_1(2+3)=\frac{\sigma_2 r_{12}+\sigma_3 r_{13}}{\sqrt{\sigma_2{}^2+\sigma_3{}^2+2\sigma_2\,\sigma_3\,r_{23}}}$$

becomes

$$r_{1\,(2+3)}\text{ (in the case of standard measures)}=\frac{r_{12}+r_{13}}{\sqrt{2+2r_{23}}}\,.$$

It is to be expected that by increasing the number of measures of both variables one may obtain higher correlations. The question arises, how high a correlation may one expect between intelligence test scores and imputed intelligence with an infinite number of repetitions of all measures? If our obtained coefficients may be assumed to be representative of all the coefficients which would occur in an infinite number of repetitions of the experiment, such a 'theoretical maximum correlation' may be ascertained by means of Spearman's process for the 'correction for attenuation[1].' Thus upon our data the theoretical maximum correlation of an infinite number of Otis tests with an infinite number of single one-rater ratings would be 0·739 with a probable error by Kelley's formula No. 161 *d* of ± 0·047, $N = 89$. If the one-rater ratings are taken as the mean of two independent ratings by each rater, this theoretical maximum correlation would be 0·749 ± 0·043, $N = 89$. This coefficient would be still further increased by taking the mean value of an infinite number of ratings by each rater, if this were possible, though the increase would probably be small.

This agreement is not high in view of the fact that each variable purports to be a comprehensive measure of intelligence. If the theoretical maximum correlation of intelligence test scores with imputed intelligence is about 0·75, how important are the other factors not measured in this correlation? Let r_{12} denote this theoretical maximum correlation, and let r_{13} denote the correlation of either given variable with the composite of all the factors not measured in the correlation r_{12}. Then since r_{23} must *ex hypothesi* be zero, the usual formula for multiple correlation[2] may in this case be written

$$R_{1(23)} \text{ (when } r_{23} = 0) = \sqrt{r_{12}^2 + r_{13}^2}\,.$$

Since $R_{1(23)}$ must equal unity, the correlation of either given variable with the unmeasured constant factors must be about 0·66. Since the half of a perfect or complete correlation from the standpoint of multiple correlation is 0·7071, *i.e.* $\sqrt{0{\cdot}5}$, the meaning is that the constant factors present in both variables, ratings and Otis test scores, are only slightly more than half-measured in the theoretical maximum correlation of

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[1] This formula may be derived simply from the Spearman sums-and-differences formula on the assumption that the obtained correlation coefficients and standard deviations are equal to those which would occur in an infinite number of repetitions of the experiment. This theoretical maximum value may best be taken as the ratio of the geometric mean of the correlations of the two variables to the geometric mean of their 'reliability' coefficients.

[2] Yule, G. U., *Introduction to the Theory of Statistics*, Griffin, 1922, p. 248, no. 15.

Correlations.

$N=89$.

	Chronological age	Rating *A* 1	Rating *B* 1	Rating *A* 2	Rating *B* 2	Otis *A*	Otis *B*	Otis *A*+*B*
Rating *A* 1	0·179 ±0·069	—	0·642 ±0·042	0·788 ±0·027	0·665 ±0·040	0·602 ±0·046	0·575 ±0·048	0·616 ±0·044
Rating *B* 1	0·361 ±0·062	0·642 ±0·042	—	0·671 ±0·039	0·819 ±0·024	0·505 ±0·054	0·513 ±0·050	0·533 ±0·051
Rating *A* 2	0·193 ±0·069	0·788 ±0·027	0·671 ±0·039	—	0·681 ±0·038	0·585 ±0·047	0·589 ±0·047	0·614 ±0·045
Rating *B* 2	0·395 ±0·060	0·819 ±0·040	0·819 ±0·024	0·681 ±0·038	—	0·567 ±0·048	0·509 ±0·053	0·563 ±0·049
Ratings *A* 1+*B* 1	—	—	—	—	—	0·611 ±0·045	0·600 ±0·046	0·634 ±0·043
Ratings *A* 2+*B* 2	—	—	—	—	—	0·628 ±0·043	0·599 ±0·046	0·642 ±0·042
Ratings *A* 1+*A* 2	—	—	—	—	—	0·628 ±0·043	0·616 ±0·044	0·651 ±0·041
Ratings *B* 1+*B* 2	—	—	—	—	—	0·562 ±0·049	0·536 ±0·051	0·574 ±0·048
Ratings *A* 1+*B* 1+*A* 2+*B* 2	—	—	—	—	—	0·644 ±0·042	0·618 ±0·044	0·657 ±0·041
Otis *A*	0·020 ±0·071	0·602 ±0·046	0·505 ±0·054	0·585 ±0·047	0·567 ±0·048	—	0·826 ±0·023	—
Otis *B*	0·156 ±0·070	0·575 ±0·048	0·513 ±0·050	0·589 ±0·047	0·509 ±0·053	0·826 ±0·023	—	—

0·75. If this maximum correlation may be assumed under ideal experimental conditions to attain the value of 0·80, the importance of the unmeasured constant factors is still represented by a correlation coefficient of 0·60. Whether the responsibility for this unsatisfactory amount of agreement lies in the ratings or in the intelligence tests cannot yet be answered. Disproof is easier than proof in the field of intelligence test validation. One can only say that *both* of them cannot be considered as adequate measures of the *true intelligence* which each attempts to measure. It is also possible that *neither* of these are valid measures.

However, this need not mean that the actual usefulness of either the test scores or the ratings is to be questioned. The reliance placed upon both forms of estimate in education, in social work, in psychiatry and in industry is probably sufficiently justified by results. It may be only that the fault lies in the use of the word *measure* in reference to intelligence. In the physical sciences one is dealing with measures whose 'reliability' (*i.e.* the correlation or agreement between the results of successive observations made by the same observer) and whose 'objectivity' (*i.e.* the correlation or agreement among the results obtained by different observers) ordinarily lie close to unity, though in actual observations never actually reach unity, while the intelligence measures, whether taken as subjective ratings or as the scores from the many published 'intelligence scales,' usually show reliability and objectivity correlations ranging from the sixties or seventies to the nineties. The essential nature of measurement is probably the same in both fields, the only differences being quantitative and not qualitative. If the term *measurement* can be considered as including reliability and objectivity correlations as low as the sixties or seventies, we may be correct in saying that either ratings or test scores are truly measures of intelligence but of a poorer quality than in the case of the traditional physical measures.

The writers wish to express their gratitude to Mr C. Moore, formerly Headmaster, and Mr H. Wackett, Assistant Master, of the Oxford Central School for their co-operation in this experiment.

(*Manuscript received* 15 *June* 1926.)

EDWARD BRADFORD TITCHENER
1867–1927

By CHARLES S. MYERS.

By the death of Prof. Titchener Experimental Psychology has lost one of its first British founders. Although he spent the last thirty-five years of his strenuous life in the United States, he remained an Englishman throughout its sixty years. He had but one teaching post—at Cornell University, Ithaca; and there he died on August 3rd last, from a cerebral tumour, after a brief illness.

Edward Bradford Titchener came of an old Chichester family. He was born at Chichester on January 11th, 1867. We hear of a certain John Tychenor being headmaster of the local Prebendal School in 1532. To this school went young Edward, and from it he gained a scholarship at Malvern College at the age of fourteen. In his nineteenth year he proceeded to Brasenose, Oxford, where he held a senior classical scholarship for four years. He obtained a first-class both in Moderations in 1887 and in *Literae Humaniores* in 1889. He took his B.A. at Oxford in 1890, and his M.A. in 1895; in 1906 he received the degree of D.Sc. at that University.

During the academic year 1889–90 he worked as a research student under the celebrated Oxford physiologist, Burdon Sanderson. To him he dedicated his first book. Doubtless owing to his previous studies in philosophy, Titchener became now attracted to the work of Wundt in Leipsic who, having passed from a training in physiology to the study of philosophy, had by a converse route been drawn to experimental psychology. Wundt's psychological laboratory was then eleven years old. Here Titchener worked for the next two years, with Scripture, Frank Angell, Witmer and Warren from America as fellow-students, and under Külpe as *Privatdozent*. In the eighth volume of Wundt's *Philosophische Studien* Titchener published a short paper on "Recognition." In 1892 he obtained his doctorate in philosophy at Leipsic by a dissertation, entitled *Ueber binoculare Wirkungen monocularer Reize*, which appeared in the same volume of the *Studien*.

Returning to England in that year, Titchener gave for a short time some University extension lectures in biology at Oxford. He endeavoured to find an opening for teaching experimental psychology at Oxford, but received even less academic encouragement than is accorded there to the subject to-day. Meanwhile Frank Angell, his former fellow-student at Leipsic, had opened a psychological laboratory at Cornell, but in the year following accepted an invitation to Stanford University. This set free the post at Cornell to which, on his recommendation, young Titchener was appointed.

Thus in 1892 Titchener became Assistant Professor of Psychology in Cornell. In 1895 he was made Sage Professor of Psychology there, and in 1910 his professorship was attached to the Graduate School of that University.

Throughout the thirty-five years' tenure of his professorship at Cornell, Titchener never wavered either in his enthusiastic worship of Wundt, whom, however, he had never known intimately at Leipsic, or in his loyal patriotism to Great Britain, which, however, he seldom visited because of its fancied neglect of him. He never became a Fellow of the Royal Society. As an Englishman, he was ineligible for admission to the National Academy of Sciences. He contributed, by special lectures in America, to the Prince of Wales's fund for the widows and orphans of those who had fought in the Great War. His faithfulness towards Cornell led him to decline both the offer of Münsterberg's chair at Harvard in 1917, and the Presidency of Clark University which fell vacant on the retirement of the late Stanley Hall.

Titchener's devotion to Wundt is illustrated by his heroic attempts at publishing an English translation of the *Physiologische Psychologie.* While still at Oxford, he had already translated the third edition; but on taking it to Leipsic, he found that Wundt was about to issue the fourth edition of this work. Forthwith Titchener applied himself to translating the fourth edition, only again to find that Wundt had overtaken him by preparing the fifth edition. Still undaunted, he began immediately to translate the fifth edition, part of which he published without delay in 1902. Pre-occupation with his own text-books put a stop to further labour in this direction. He had already translated Külpe's *Grundriss* in 1895 and, with Pillsbury, one of his first pupils at Cornell, Külpe's *Introduction to Philosophy* in 1897.

In 1896 Titchener published his *Outline of Psychology*; this was replaced in 1910 by his *Text-Book of Psychology.* In 1898 he brought out his *Primer of Psychology*; this was replaced in 1915 by his *Beginner's Psychology.* In 1908 he published his Columbia *Lectures on the Elementary Psychology of Feeling and Attention*; and in the following year his Illinois

Lectures on the Experimental Psychology of the Thought Processes. In 1911 he gave the Lowell Lectures at Cambridge, U.S.A., which were never published. He received the honorary degrees of D.Sc. at Harvard, Litt.D. at Clark, and LL.D. at Wisconsin.

During the years 1901–5 he issued his *magnum opus*, the four volumes of his *Laboratory Manual of Experimental Psychology.* It had demanded thirteen years' hard work, and its financial cost, he used to say, nearly ruined him. Its preparation involved the invention and standardization of many useful and now familiar psychological apparatus for laboratory teaching. The *Manual* has since been translated into most European languages and into Chinese and Japanese. It has formed the basis of many later laboratory text-books, and it led to requests for his assistance in planning psychological laboratories in a large number of countries both in the Old and in the New Worlds.

Between 1894 and 1920 Titchener acted as American Editor of *Mind.* From 1894 until 1925 he was Associate Editor and ultimately Editor of the *American Journal of Psychology.* His aim finally was to found a journal as representative of Cornell psychology as Wundt's *Psychologische Studien* had been of the Leipsic School. True to his German training, Titchener's ideal was to establish his own School. He had to some extent imbibed those earlier German methods of laboratory research, whereby the Professor or his *Privatdozent* allocated experimental tasks to their students in order to confirm the Professor's views; and woe betide the candidate for the doctorate or his *Dozent,* if those pontifical views were not corroborated by research! Free and open discussion, of course, whether by pupils or staff, was not tolerated. Such, if in caricatured outline, were the conditions in which Psychology had grown up at Leipsic and which tended to be imported into Cornell. It led inevitably to certain occasional 'differences' between Titchener and his assistants and colleagues.

Titchener was essentially a 'separatist.' He broke from the American Psychological Association and founded at Cornell a body known as the 'Experimentalists.' He broke from the *American Journal of Psychology* and was on the point of founding a new journal before he died.

Although he found it difficult to forgive an enemy, no one could rival Titchener in his kindness and generosity to his friends. The *Festschrift,* presented to him in 1917 after twenty-five years' teaching work at Cornell, is sufficient indication of his far-reaching influence. By 1923 the number of papers for which he was personally responsible written by his students amounted to 158, while his own published articles numbered 190 by the same date. He had finally an annual entry of 1000 undergraduates, and a staff of two professors, two instructors, and three assistants. At the time of his death he was engaged on a *Systematic Psychology* which was to appear in three or four volumes; of these he had practically completed the first before his fatal illness.

Titchener's attitude towards psychology showed relatively little change throughout his life, despite the rapid progress and development of the subject. He took little interest in individual mental differences, in Behaviourism, the *Gestaltpsychologie*, mental testing, or in applied psychology. His single aim was to study the human mind *in general*, and to express all mental processes in terms of a fixed number of hypothetical elements and attributes. Although such atomistic procedure has been largely superseded now as a psychological ideal, Titchener's views were useful in their day in provoking much experimental research. Whether or not they ever regain their former influence, their value during a by-gone stage in the development of psychology is unquestionable.

PUBLICATIONS RECENTLY RECEIVED

Elementary Conditions of Human Variability. By RAYMOND DODGE. New York: Columbia University Press. Publication No. 10 of the Ernest Kempton Adams Fund for Physical Research. Pp. xiii × 107. $ 1.50.

The object of the investigation described in this monograph was the quantitative determination of the changes that occur in human reactions, at different levels of the nervous system, in response to the repetition of identical stimuli. The technique was developed in a series of researches from 1895 onwards, and a model series of observations were carried out from 1915–1917, accurate photographic methods of recording being used. The chief reactions that were measured were representative of many levels of neuro-muscular processes, and were chosen for their wide distribution and relative simplicity, reliability and freedom from voluntary control. The main results show the importance of the refractory phase at all levels, and throw considerable light on the problems of habituation and learning, also on the natural physiological rhythms of human efficiency. Although they may seem not to be very conclusive, and suffer from being obtained on only one subject, yet undoubtedly the study is a most significant piece of work for physiology and psychology, since it is almost the first attempt at real physiological research on the intact central nervous system.

Speaking with Tongues: Historically and Psychologically Considered. By GEORGE BARTON CUTTEN. New Haven: Yale University Press. (London: Humphrey Milford), 1927. Pp. xii + 193. 11*s.* 6*d.* net.

Dr Cutten has written a lucid and interesting book giving an account of a large number of instances of the "gift of tongues" from New Testament times onwards to the present day. He shows how the necessary conditions are (*a*) a considerable amount of internal and external excitement, (*b*) suggestion in some form, and (*c*) in most cases a degree of illiteracy. There are four forms of "speaking with tongues": (1) production of inarticulate sounds; (2) production of articulate sounds which simulate words; (3) production of fabricated or coined words; and (4) actual speech in a foreign tongue. In the fourth form the speaker always uses a language with which he has come in contact, even though he cannot, when fully conscious, volitionally produce words in that language.

Dr Cutten has not only collected together a number of most attractive illustrations of the curious occurrence with which he deals, but has given a pretty convincing psychological explanation, without degenerating into unnecessary technicality. He adds a good bibliography. The book should be read with interest by any student of psychology and particularly by those concerned with the psychology of religious expression.

The Psychology of Character, with a survey of temperament. By A. A. ROBACK. London: The International Library of Psychology, Philosophy, and Scientific Method; Kegan Paul, Trench, Trubner and Co., Ltd. 1927. Pp. xxiv + 595. 21*s*. net.

This comprehensive study of character, and inevitably also of temperament and personality, is divided into four parts. In the first part the historical background is surveyed. The second part deals with the various classifications of character: in it Fourier's treatment and the views of British, French, Teutonic, Dutch, Russian, Hungarian, Italian and South American writers are considered. The third part is devoted to "Movements and Methods." The contributions made by psychiatry, psycho-analysis, the *Gestalt* school, the endocrinologists, etc., and the various attempts which have been made to attack the subject experimentally are outlined; and the value of the study of histories and biographies is assessed. In the last part Roback puts forward his own views.

Roback sees no reason why there should not be a general factor of character and specific sub-factors. He defines character as an enduring psycho-physical disposition to inhibit instinctive impulses in accordance with a regulative principle—the 'regulative principle' being of prime importance. Asceticism, for example, is not of a high order, since the inhibitions are probably rooted in some instinctive core; not in reason. From this it follows that the highest types of character can be realized only by those of the highest intelligence, and thus we get a scheme for charting character according to the sanctions which regulate the individual's conduct.

There are many matters about which opinions may differ and others where the argument is not convincing, *e.g.* the inconsistency of woman as compared with man, and its explanation; that murderers rarely bring themselves to believe that they will not be executed (cf. Bjerre's opinion of the contrary in *The psychology of murder*); that the typical neurotic has very little character; that war neuroses were confined to those who were on the lower intellectual and moral levels; that "sugar loving and candy-eating" people are more fickle and less reliable than others; that all temperaments are on an equal footing as regards character; that scientific labours would cease if the scientist had not faith in progress; that Roback's "inhibition" is not synonymous with integration or sublimation.

The work is interesting and, though rather long, will be welcomed by those who are engaged in the difficult task of studying temperament and character.

Father or Sons? By PRYNCE HOPKINS. Published by Messrs Kegan Paul, Trench, Trubner and Co., Ltd. Pp. xv + 252. 12*s*. 6*d*. net.

After a few remarks of an introductory nature in which he expounds some of the main doctrines of the Freudian system, Prynce Hopkins proceeds to a study of social problems in the light of them. Since he accepts the main body of psycho-analytic teaching his book is mostly occupied with a study of the wider social effects of the Oedipus situation.

He divides his book into two parts, the Contra-Father motives, and the Pro-Father motives being dealt with separately. Thus in the first part we find the Rescue Phantasy, the phantasy of the father as Aggressor, and as Monopolist-Capitalist, the Castrating Phantasy, Ambivalence, and the God-Complex illustrated on the level of economic, political, and social events. In the second part we find Anxiety, Persecution of Sons, Homosexuality, Providence, Loyalty, and Authority treated on the same basis.

Prynce Hopkins' exposition is clear and to the point, and it does not contain an excess of difficult theory, but is profuse in illustration. He has a great ability to point out the appropriateness of the instances he uses, much imagination, and a gift for collecting together facts and quotations in order to support his argument.

L'Intelligence. By B. Bourdon. Bibliothèque de Philosophie Contemporaire. Libraire Felix Alcan, Paris. Pp. 388. 30 francs.

In this book a somewhat painstaking review of the various materials of conscious life is made. The author is primarily interested in observation rather than sheer introspection, or "l'imagination et le raisonnement," as he styles it, but he is nevertheless unable to regard psychology as a dynamic study; it remains on the level of analysis and description, and in discussion of the manner of the organization, connection, and interaction, of the materials of mental life, rather than becoming a study of the function of mental processes in the determination of behaviour.

La Pensée Concrète. By Albert Spaier. Libraire Felix Alcan, Paris. Pp. 446. 45 francs.

During the course of an "Essai sur le Symbolisme Intellectuel" the author has studied, in the first place, the relationship of images to thought, the types of images found in thinking, and their function; and in the second place, the importance of freedom in rational processes. He discusses the views of many writers from Descartes and Spinoza up to Ward; and even the contemporary "*Gestaltpsychologie*" finds a place. Finally he proceeds to conception and abstract thought. In the second part he maintains the importance of "*La Raison Libératrice.*" We are not to be allowed to reduce freedom to autonomy.

Colour Music: The Art of Light. By Adrian Bernard Klein, London: Crosby Lockwood and Son, Stationers' Hall Court, Ludgate Hill, E.C. 1926. Pp. xvi + 287. 36*s.* net.

The term 'Colour Music' is perhaps misleading. It is based on a false analogy with the art of music, but it has been maintained since it describes, with more or less accuracy, the idea of the art—the conveyances of ideas and emotions and the arousal of esthetic feelings by the production of combinations and, more especially, of sequences of colours.

It is over 200 years since a Jesuit Monk first attempted to make an instrument for the performance of Colour Music, but little was achieved till recently owing to the practical physical difficulties of lighting and illumination. Recent scientific progress has surmounted many of the earlier defects, and a number of 'concerts' have already been given.

In this book Mr Klein, who has himself done much experimenting, describes the various hypothesis which have been suggested and the instruments have been made.

He also discusses the claim that Colour Music is an independent art, its relation with other arts, the psychological work which has a bearing on it, and stage lighting.

The work has been well done, and should be read by all those who are interested in Colour Music.

James Ward 1843–1925. London: Published for the British Academy by Humphrey Milford, Oxford University Press. 1927. Pp. 11. 1*s*. net.

In this brief article Professor W. R. Sorley writes of the life and work of the late Professor James Ward. The story, as everybody who has read the brilliant memoir by Mrs Campbell prefixed to the lately published ***Essays in Philosophy*** (C.U.P. 1927) is one of enthralling interest. Here it is told sympathetically and with the utmost clarity. Professor Sorley adds a short critical appreciation of Ward's work in psychology and philosophy, in which the leading points and motives of both are thrown into bright relief.

How to Succeed in College. By William F. Book. Baltimore: Warwick and York. 1927. Pp. 192.

What particular college is meant is not indicated, but perhaps it is hoped that the growing standardization of educational methods may universalise the conditions of "success." The author attempted to find out exactly how college students make use of their terms at various stages of their career, and to compare differences with success both in academic examinations and also in later life. He also gives results of a special "How to Study" course given to College Students. There is a great deal of common sense in what he says and his book should be interesting and in various ways useful to any person who is concerned with the organization of institutions of higher education.

Sex Freedom and Social Control. By Charles W. Margold. Chicago: University of Chicago Press. 1926. Pp. xi + 143. 10*s*. net.

Dr Margold's essay is of very considerable interest to the social psychologist. The claim is that sexual relationships are in all cases subject to direct social conditioning. Consequently the distinction often made between sexual unions for the purpose of procreation and for that of the mere satisfaction of individual desire is neither psychologically not sociologically sound. The arguments are well and judiciously documented and a very good bibliography is included.

Repetitorium der Practischen Neurologie. Von E. Siemerling. Leipzig: Georg Thieme. 1927. S. 172. M. 4.80.

This is a very condensed but useful revision course for the student of neurology on the clinical side. No diagrams are included.

PSYCHOLOGICAL ABSTRACTS

An international abstract journal published monthly in English by the American Psychological Association under the editorship of Walter S. Hunter and R. R. Willoughby of Clark University, Worcester, Massachusetts, U.S.A., with the cooperation of F. C. Bartlett, Cambridge, England; Ed. Claparède, Geneva; G. C. Ferrari, Bologna; A. Michotte, Louvain; H. Piéron, Sorbonne; M. L. Reymert, Wittenberg College; W. Wirth, Leipzig; and P. Ranchburg, Budapest.

Volume I, 1927, contained 2730 abstracts with a detailed subject and author index. These abstracts were of books and of the articles contained in 267 periodicals. Volume II will be enlarged. The classification of titles is as follows: General; Sensation and Perception; Feeling and Emotion; Attention, Memory and Thought; Nervous System; Motor Phenomena and Action; Plant and Animal Behavior; Evolution and Heredity; Special Mental Conditions; Nervous and Mental Disorders; Social Functions of the Individual; Industrial and Personnel Problems; Childhood and Adolescence; Educational Psychology; Biometry and Statistics; and Mental Tests.

The subscription price is $6.25 and should be sent to:

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New Jersey, U.S.A.

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COMMITTEE FOR RESEARCH IN EDUCATION

With a view to facilitating the general co-ordination of research in education, the Committee has made its annual inquiry as to researches in progress, or planned, at the various Psychological Laboratories and Training Departments.

The following is a summary of the replies received:

RESEARCHES IN PROGRESS OR PLANNED, DECEMBER, 1927.

UNIVERSITY OF BIRMINGHAM (EDUCATION DEPARTMENT).

Tests of Higher Mental Processes. (Prof. VALENTINE.)

Studies in the Psychology of Early Childhood. (Prof. VALENTINE.)

A Statistical Enquiry as to the Correlation of School and University Successes. (Prof. VALENTINE.)

The Reliability of Intuitive Judgments of Character. (Prof. VALENTINE.)

Preferences of Children and Adults in Poetry and Prose Literature. (Mr A. E. CHAPMAN.)

An Experimental Enquiry into the Suitability of the Otis Group Tests of Intelligence for use with Matriculated Students. (Miss F. M. RITCHIE in collaboration with Dr J. G. McGREGOR.)

Enquiry as to the Reasons for the Choice of Occupation among Secondary School Pupils. (Miss F. M. RITCHIE.)

The Development of Humour in Children. (Mrs L. E. SIMPSON.)

Studies of the Heterokontae. (Dr E. M. POULTON.)

The Idea of Beauty and its Development in Visual Art. (Mrs D. M. LALL.)

Formal Training, a historical and critical review. (Mr E. C. CULL.)

UNIVERSITY OF CAMBRIDGE.

Statistical Studies in Intelligence and Temperament. (Mr CLARKE.)

Methods in the study of Temperament. (Mr ROSS MACFARLAND.)

Plateaux in a Learning Curve. (MRS DRURY SMITH.)

Psychological effects of Vibration and Noise. (Miss LORRAIN SMITH.)

Vocational Guidance. (Mrs RAMSEY.)

Appreciation of Music. (Mr P. E. VERNON.)

Perceptual processes in Reading. (Mr PICKFORD.)

Physiology and Psychology of Reading. (Miss M. D. VERNON.)

DUBLIN, TRINITY COLLEGE (TRAINING DEPARTMENT).

An Enquiry into the Principle of Individuality in Education.

UNIVERSITY OF DURHAM (EDUCATION DEPARTMENT).

The Examination System in England, the psychological consideration of its value, and of the alternative methods of examination. (Mr VERNON BROWN.)

UNIVERSITY OF EDINBURGH (GEORGE COMBE PSYCHOLOGICAL LABORATORY).

A Scale of Performance Tests devised primarily with reference to the Deaf. (Dr DREVER, Dr COLLINS, Dr FAIRBAIRN.)

The Development of Moral and Religious Ideas in Children. (Mr ROSS MILLER.)

UNIVERSITY OF LEEDS (EDUCATION DEPARTMENT).

Two enquiries into perseverating tendencies.

The place of imagery in the teaching of English.

UNIVERSITY OF LONDON.

Bedford College.

A study of children's judgments.

Correlation of the "June Downey" tests with tests for practical ability and with school ratings.

Tests for the detection of eidetic imagery in school children.

Study of the relation between sheer facility for words and intelligence as measured by Binet tests.

King's College.

Literary Appreciation.

Retentivity.

Influence of Conation on Mental and Manual Operations.

University College.

Development of Concepts.

Application of Rules and Examples.

Bi-lingual education.

Tests of Mathematical Aptitude.

Perception of Children.

Individual Differences of Ability.

Mental Oscillation.

Fluctuations in Mental Output.

Effect of Practice on Mental Testing.

Learning and Forgetting.

Construction and Efficiency of Mental Tests.

Mental Inertia.

The Factor of Speed in Intelligence.

UNIVERSITY OF MANCHESTER.

Investigations into the Acquisition of Muscular Skill.

Investigation into the Transfer of Training in Manual Dexterity, and of Visual Discrimination.

Comparative Study of Vocational Tests that involve Skill and Dexterity.

Study of the Whole and Part Methods in Acquiring Skill.

NATIONAL INSTITUTE OF INDUSTRIAL PSYCHOLOGY.

The Use of Psychological Tests in Vocational Guidance.

(*Carnegie Experiment carried out in London Schools.*)

The follow-up stages are now in progress. Reports on the results obtained from the use of special tests are in preparation and will be published shortly. These include:

(*a*) Performance Tests of Intelligence at school leaving age.
(*b*) Tests of Mechanical Ability.
(*c*) The Problems of Distributed Attention.
(*d*) Tests of Manual Dexterity.

Research Work Planned:

Vocational Guidance. An experiment in Vocational Guidance is planned in the County of Fife in which psychological tests will be given at regular intervals beginning at the age of eleven until the vocational decision is made. Arrangements for follow-up are contemplated as in the London experiment.

UNIVERSITY OF SHEFFIELD (EDUCATION DEPARTMENT).

A Study of Children's Vocabularies.
The Theory of Correlation.
A Study of Motor Ability.

UNIVERSITY OF ST ANDREWS (EDUCATION DEPARTMENT).

The Development of Literary Appreciation in School Children.

NINTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

The Ninth International Congress of Psychology will be held at Yale University in New Haven, Connecticut, U.S.A., probably in August or September 1929.

The officers of the Congress are as follows: President, J. McKeen Cattell of New York; Vice-President, James R. Angell of Yale University; Secretary, Edwin G. Boring of Harvard University; Treasurer, R. S. Woodworth of Columbia University; Foreign Secretary, Herbert S. Langfeld of Princeton University; Executive Secretary, Walter S. Hunter of Clark University; Chairman of the Program Committee, Raymond Dodge of Yale University; Chairman of the Committee on Arrangements, R. P. Angier of Yale University.

Besides these men the National Committee includes J. E. Anderson, University of Minnesota; Madison Bentley, University of Illinois; E. A. Bott, University of Toronto; H. A. Carr, University of Chicago; Knight Dunlap, Johns Hopkins University; S. W. Fernberger, University of Pennsylvania; William McDougall, Duke University; W. B. Pillsbury, University of Michigan; C. E. Seashore, University of Iowa; L. M. Terman, Stanford University; E. L. Thorndike, Columbia University; H. C. Warren, Princeton University; M. F. Washburn, Vassar College; R. M. Yerkes, Yale University.

This is the first meeting of the Congress in America. The previous meetings have been as follows: Paris, 1889; London, 1892; Munich, 1896; Paris, 1900; Rome, 1905; Geneva, 1909; Oxford, 1923; Groningen, 1926. It is expected that the Congress in the United States will be truly international in character. The Americans hope that the appointment of some foreigners for lecturers and lectureships can be arranged near the time of the Congress, so that foreign attendance can be increased and international solidarity within psychology furthered still more. Most appointments of this kind at American universities would have to apply only to psychologists who speak English.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

January 14, 1928.	"The Influence of the Menstrual Cycle on Muscular and Mental Efficiency." By Dr C. S. Myers, F.R.S.
	"Mechanical Aptitude: Its Existence and Measurement." By Mr J. W. Cox, M.Sc.
February 11, 1928.	"Factors in the Pathology of Work—Laziness and Stupidity." By Prof. T. H. Pear.
	"The Present Position to the Concept 'Transfer,' and its relation to 'Transference.'" By Miss Edna M. Yates, B.Sc.
	"An Experimental Investigation into the Transfer of Training in Manual Dexterity." By Mr J. N. Langdon, B.Sc.
March 3, 1928.	"A Note on the Theory of 'Shape' in the light of some Recent Experimental Work." By Dr Saidullah.
	"The Evaluation of the Heterogeneity of the Binet Tests and the Resulting Fallacy of the I.Q." By Mr E. R. Clarke, B.A.

SECTIONAL MEETINGS.

EDUCATIONAL.

January 2, 1928.	"Those First Five Years." By David Forsyth, M.D., D.Sc., F.R.C.P.
January 26, 1928. (Joint General and Education Meeting)	"La Globalisation dans L'Ecriture et la Lecture." By Dr Ovide Decroly.
February 13, 1928.	"The Child's Early Discrimination between Sound and Speech." By Miss Mary Chadwick.

MEDICAL.

January 25, 1928.	"A Physiological Approach to the Problem of the Unconscious." By Dr D. N. Hardcastle.
February 22, 1928.	"Conditioned Reflexes and Experimental Neuroses." By G. V. Anrep.

INDUSTRIAL.

February 8, 1928.	A discussion on: "Occupational Neuroses." By Dr Millais Culpin.

CAMBRIDGE: PRINTED BY
W. LEWIS, M.A.
AT THE UNIVERSITY PRESS